THE AMERICAN JOURNAL OF SOCIOLOGY is published bimonthly in July, Sept
November, January, March, and May by The University of Chicago at The Uni
of Chicago Press, 5801 Ellis Avenue, Chicago, Illinois 60637 Subscription rates, U
institutions, 1 year $15 00, 2 years $29 00, 3 years $43 00; individuals, 1 year $
2 years $19.00, 3 years $28 00. Student subscription rate, U S A · 1 year $6 00
from professor must accompany subscription). All other countries add $1 00 fo
year's subscription to cover postage. Single copy rates: current volume, instit
$3.50, individuals $2 50; back issue, institutions $4 00, individuals $3 00 Back
available from 1963 (vol 69). Subscription rate to members of American Soci
Association: 1 year $7 50 Subscriptions are payable in advance Please make
mittances payable to THE AMERICAN JOURNAL OF SOCIOLOGY, The University of C
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OF SOCIOLOGY, 1130 East 59th Street, The University of Chicago, Chicago, Illinois
Business correspondence should be addressed to The University of Chicago Pres
Ellis Avenue, Chicago, Illinois 60637. *Claims for missing numbers* should be made
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serve stock will permit *The articles in this Journal* are indexed in the *Social Scien*
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available *in microfilm* from University Microfilms, 300 North Zeeb Road, Ann
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PRINTED IN U.S A

AJS

American Journal of Sociology

Volume 77 No. 1 July 1971

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Kenneth Kessin is an assistant professor at Rutgers University. His current research centers on the determinants of personal well-being and loneliness. He is also working on a text emphasizing descriptive and inductive sociology.

Barbara Laslett is an assistant professor of sociology at San Fernando Valley State College. Her main interests are in the fields of social stratification and the family.

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American Journal of Sociology

Volume 77 No. 2 September 1971

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Social and Psychological Consequences of Intergenerational Occupational Mobility[1]

Kenneth Kessin
Rutgers University

Studies relating intergenerational mobility to disturbed emotional states and decreased participation in solidary groups present contradictory evidence. Recent theoretical work suggests that the relationship between mobility and its hypothesized detrimental consequences will hold to a greater extent in a traditional and static social order and to a lesser extent in a society already "modernized." Aside from conflicting empirical findings, methods used to determine the effects of mobility have been unable to control simultaneously for prior and current socioeconomic level. Using dummy-variable multiple-regression analysis, scores for Community Integration, Primary Affiliation, Family Participation, Manifest Anxiety, and Psychosomatic Symptoms show few overall systematic effects of mobility. Respondents moving upward two or more socioeconomic levels have significantly lower Community Integration scores and significantly higher Manifest Anxiety and Psychosomatic Symptom scores. Scores on the dependent variables are unaffected by moderate amounts of either upward or downward mobility. Among very downwardly mobile respondents, the scores are the reverse anticipated by theory. The results and implications for mobility theory are discussed.

INTRODUCTION

What are the social and psychological consequences of intergenerational mobility? Tumin (1967), among others, believes the question is answerable. Among the consequences of mobility he lists overconformity, lower levels of family cohesion, belief in an open class structure, and feelings of familial rejection on the part of the upwardly mobile. Among the downwardly mobile, political conservatism, acceptance of the class system, lower levels of family cohesion, and anti-integrationist attitudes are characteristic. Tumin (1967, p. 97) concludes that the concept of mobility and the empirical studies examining the detrimental consequences of mobility are sociologically viable:

> The general trend in these findings is that the mobility experience in a status-minded society is likely to have some disruptive consequences, either because of the status orientation or anxiety of the mobile indi-

[1] The author wishes to express his appreciation to Profs. C. Arnold Anderson, Norman M. Bradburn, James A. Davis, Anne Foner, and Edward E Nelson for their assistance throughout the present research. This research is a revised version of my Ph D. dissertation (Kessin 1967).

> vidual, or because of his inability to adjust successfully to the new groups into which he moves, whether up or down

For Tumin, and we may add Janowitz (1956), Blau (1956), and Lipset and Bendix (1959), social mobility is associated with anxiety, tension, and difficulty in adjustment. In sum, the consequences of mobility are both socially and psychologically disruptive for the mobile individual.

This is not to say that there has been a totally uncritical and unequivocal acceptance of theory and data in this area. Rather, these writers represent a view that mobility is a useful concept and that the empirical research generally confirms the disruptive effects hypothesized by theory.

Other writers have examined the data, the theory of the consequences of individual mobility, and the conceptualization of mobility and its measurement and find all three wanting. "For reasons of both theory and method, intergenerational mobility may be the least promising variable in stratification research," Wilensky (1966) concludes. Going a step further, Duncan, among others, has pointed to a major methodological deficiency in the field. He says that if all previous empirical work were in perfect agreement and there were no problems in the measurement of mobility, the inability of previous research efforts to control prior and present SES simultaneously renders conclusions about the "effects" of mobility inconclusive (Duncan 1966).

In the first section of this paper, a brief evaluation of both theory and measurement of mobility will be presented. Second, Duncan's methodological statement will be discussed in greater detail. The third section will review previous research findings, focusing on studies relating individual mobility to participation in informal groups and to variations in levels of emotional adjustment. Then the findings of the present study will be introduced. We will adopt Duncan's suggested method in an attempt to resolve and clarify the evidence concerning the social and psychological consequences of individual mobility.

Theoretical and Operational Problems in the Study of Mobility

Theories and research hypotheses which anticipate detrimental consequences of individual mobility have been based, explicitly or otherwise, on certain assumptions about the society in which mobility takes place.[2] Disruptive consequences can be anticipated if: socialization to the new status is inadequate; the magnitude of movement is extreme; the stratum of arrival or departure rejects the mobile person, or the newcomer rejects the new or old stratum; geographic mobility accompanies social mobility;

[2] I have discussed these assumptions and their empirical validity more fully (Kessin 1967).

mobility is a unique event (mobility rates are low); social status is highly salient; one's mobility and one's origins are visible.

Germani (1966), writing in a similar vein, emphasizes the need to consider the context in which mobility occurs. Litwak (1960) has shown how changes in family structure, from a "classical" to a "modified-extended" form, may alter the presumed consequences of mobility. Wilensky (1966) sees potentially disruptive effects of personal mobility declining to the extent that (1) channels of mobility and wealth increase and (2) career lines become more orderly.

In summary, these writers stress the importance of structural, institutional, and cultural contexts in which mobility occurs. Our argument is that detrimental effects of mobility are most likely to be found in traditional, static, class-homogeneous societies with low rates of personal mobility. Because the available empirical studies are not always in agreement and because psychological and social maladjustment are not always associated with mobility—in other words, because the data by no means show clear-cut patterns—these points cannot be ignored.

Far too often, research has proceeded by defining mobility as movement along an occupational rating scale. Miller (1955) and Curtis (1961), among others, have discussed the multifaceted nature of SES and have criticized measurement of either SES or mobility based on a single index. The field has almost exclusively used a single index to measure status. From this arises the problem of defining mobility. Is movement from one adjacent category to another "mobile"? Is a one-point change on a continuous ranking scale enough movement to define someone as mobile? These questions just begin to suggest sociology's need to grapple with the conceptualization of mobility and the adequacy of its operationalization.

The Question of Research Method

Duncan (1966, p. 91) has stated: "One is not entitled to discuss the 'effects' of mobility . . . until he has established that the apparent effect cannot be due merely to a simple combination of the effects of the variables used to define mobility."

In studying the effects of mobility upon social and psychological life, we must examine the effects of the statuses of origin and destination simultaneously operating on the dependent variable. For any effects of mobility to be demonstrated, these effects must be evident over and beyond those effects which can be accounted for by prior and present status.[3] Duncan recommends use of dummy-variable multiple-regression

[3] This discussion and the procedure suggested for solving the problem consider mobility to reflect a status-discrepant condition (see Blalock [1966] for a discussion of this point).

equations. The two independent variables are present and prior SES. The multiple-regression equation is used to estimate predicted values of the dependent variable. Predicted values are then compared with observed values. Where discrepancies are observed, evidence is provided for the effects of mobility over and beyond the "additive" effects of the two independent variables.

Virtually all past research designs in the field of mobility studies have been unable to control for the simultaneous effects of the two SES variables. Typical designs have called for a comparison of stable versus down- or up-moving aggregates. Occasionally, a mobile group is compared with a stable group in the stratum of arrival and a stable group from the stratum of departure. Most have been cross-sectional rather than longitudinal in design.

The Consequences of Intergenerational Mobility

In the present study, dummy-variable multiple-regression analysis was employed to examine the effects of mobility on family relationships, interpersonal and communal relationships, and emotional adjustment. Social isolation, family stress, and emotional disorder have been discussed, alternatively, by what Ellis and Lane call the dissociative and compensatory hypotheses (Ellis and Lane 1967). The dissociative hypothesis, stemming from the work of Sorokin, sees mobility as a disruptive process. The mobility experience leaves a man isolated, detached, lonely, and emotionally distressed. In the compensatory theory, the direction of causality is reversed Emotional and interpersonal difficulties precede social movement in time. Fromm (1941) and Horney (1937) see isolation and loneliness as the outcomes of unfavorable family relationships. Upward mobility is a compensatory device for attaining the support, recognition, and love which was denied in the formative years. The present research is not concerned with testing these models, although where appropriate, comments on their "fit" to the data will be made.

Empirical Evidence: Family Identification, Tension, and Interaction

Empirical studies reveal a varied but generally consistent set of findings with respect to the emotional climate in the family. Dynes, Clarke, and Dinitz (1956) reported, among aspiring boys, feelings of rejection by family. The use of nonfamily persons as sources of orientation or identification by aspiring boys has been reported by Eulau and Koff (1962), Simpson (1962), and Ellis and Lane (1963). Conflict between parents

and children over careers (Ellis 1952) and values (Himes 1952) have been reported, while detachment of sons from family is a pattern observed by Levine and Sussmann (1960) and Douvan and Adelson (1958). In the latter study, however, the authors concluded that the autonomy observed among aspiring boys was not pathological. Their autonomy was healthy and mature in contrast to evidence suggestive of ambivalence, detachment, and feelings of rebellion and rejection voiced by downwardly aspiring boys.

Generally lower rates of interaction with family and/or extended kin have been reported by Lemasters (1954), Stuckert (1963), and Curtis (1958). Litwak (1960), however, observed no differences in interaction rates, while Adams (1968) noted strong evidence of detachment among the downwardly mobile, especially among women. A recent study of family participation, employing statistical procedures similar to those outlined by Duncan, observed no effects of mobility, either upward or downward, which cannot be attributed to the additive effects of prior and present SES (Aiken and Goldberg 1969).

Overall, the data suggest stressful emotional relationships, tension, and parent-child estrangement associated with actual or anticipated mobility. But with respect to rates and patterns of familial interaction, no general trend is observable from the reported data

Inability to Form, Maintain, or Retain Intimate and Informal Relationships

Warner and Abegglen's study *Big Business Leaders in America* (1956, p. 90) sets the tone for this section. "All of these mobile men, as a necessary part of their equipment, . . . have difficulty in accepting and imposing the kinds of reciprocal obligations that close friendships and intimate contacts imply. They typically are isolated men." This difficulty in maintaining intimate social ties among the upwardly mobile was not confirmed, at least in the context of marriage. Roth and Peck (1951) reported the highest rates of marital satisfaction among the upwardly mobile. Curtis (1958) observed the lowest rates of divorce in this group In both studies, marital satisfaction was lowest and divorce rates highest among the downwardly mobile.

Simpson's study of aspiring and nonaspiring boys observed a preference by mobile aspirants for interaction with higher-status boys rather than boys of their own level. By contrast, Curtis (1959*a*) was unable to observe differences in interaction rates with friends, neighbors, and work associates between mobiles and stables. Further, mobility was unrelated to participation in voluntary organization (Curtis 1959*b*). More recently,

Curtis (1963) found that the upwardly mobile retained friends from their former strata.

Ellis and Lane's (1967) study of very mobile boys observed them to be (1) difficult to get to know, (2) socially isolated, and (3) not active in extracurricular activities. These patterns, observed in freshmen at Stanford, persisted through their undergraduate careers. Crockett (1962) found lower affiliation-need scores among upwardly mobile boys.

The available evidence points to interpersonal difficulties among mobile aspirants and when completed mobility represents a major status change. Benyon's study of immigrants from Hungary who, if they lost status, broke their former relationships is the only research on the effects of downward mobility (Benyon 1936).

Emotional Adjustment and Mental Health

Ellis's work (1952), cited earlier, reported self-reports of more severe and more numerous psychosomatic symptoms among mobile career women. Other research is inconclusive. Lystad (1957) observed downward mobility among schizophrenics, while Hollingshead and Redlich (1955) observed that upward mobility and schizophrenia were linked, with the lowest rates of mobility among a group of normal subjects.

For more than a decade, these three studies of mobility and mental illness have served as the evidence for theory. We feel little confidence in generalizations from patient populations or from a limited number of career women. However, more recent work permits greater confidence in empirical observations.

The Midtown Manhattan study has provided two major reports on emotional adjustment. Srole et al. (1962) observed higher rates of emotional impairment among the downwardly mobile and lowest rates among the upwardly mobile. Langner and Michael (1963) reported similar results, although it must be kept in mind that the same population was being analyzed in these two reports. Both works question any causal connection between mental health and mobility, although Langner and Michael suggest that downward mobility may be the result instead of the cause of poor emotional health. Empirical confirmation of better emotional adjustment of the upwardly mobile is found in Douvan and Adelson's (1958) study of aspiring boys. On six measures of emotional health, the aspirers displayed significantly better performance.

Our review certainly does not support an unequivocal view. If anything, the downwardly mobile may be suffering the misfortunes of status descent, while the upwardly mobile appear to be better off for it. Given the problems of design discussed earlier in this paper, we must still treat any empirical generalization with caution.

Summary of the Empirical Data

Ordinarily, a literature reviewer seeks some common ground, orders available data, and hopefully arrives at a position whereby theory and fact extend themselves and intertwine. Having reviewed the available data and discussed the problems of research design, I do not feel it is reasonable to draw inferences or to formulate new hypotheses. The main objective of the research presented is a test of the simplest of questions: is there any relationship, regardless of direction, between mobility and interpersonal and psychic stress?

METHOD

Earlier, we noted the failure of mobility research to consider both prior and present status of the respondent when we are seeking to determine the "effects" of mobility on some dependent variable. Duncan has shown how differential fertility rates of mobile married couples, when prior and present statuses are taken into account, reduce the mobility effect to nil. The technique for inquiring into the effects of mobility is multiple-regression dummy-variable analysis. In this method, a multiple-regression equation based on current and past SES is computed. Respondents are cross-classified in a $j \times k$ matrix according to their current and prior status. In each cell, the observed mean score on the dependent variable is computed. The multiple-regression equation is derived from past and present SES and is then used to predict the expected scores along the same dependent variables for each cell. Predicted means are compared with observed means. Marked discrepancies between the observed and predicted means are noted, and if these discrepancies are large and/or systematic, evidence for the effects of mobility, independent of past and current SES, is revealed.

The Sample and Measurement of Independent and Dependent Variables

The population to be studied consists of 546 adult males in a Washington, D.C., suburb drawn from a larger probability sample. The men included in the present study are those for whom adequate occupational information for the respondent and his father is available. The vast majority of the study population is white (93 percent); 92 percent have Protestant or Catholic affiliation. The occupation of the respondent and his father was coded into one of four categories: Class I is professional and technical occupations; Class II is managers, officials, proprietors, clerical, and sales workers; Class III is craftsmen and foremen; Class IV

includes farm laborers, semi- and unskilled workers, private household workers, and operatives.

Five Guttman scales and quasi-scales employing the Cornell technique (Guttman 1947) were constructed to measure integration and emotional adjustment The Integration scales measure frequency and extent of participation in three informal relationships. Community Integration has five items, with a coefficient of reproducibility of 99 percent. Primary Affiliation measures the frequency and intensity of participation with friends. The scale has four items with a reproducibility of 88 percent. Family Participation is measured by a scale of three items, with a coefficient of reproducibility of 99 percent.

Emotional maladjustment is measured by means of two highly interrelated scales. The Psychosomatic Symptom scale has five items, all of which refer to psychosomatic complaints. Its reproducibility is 84 percent. The Manifest Anxiety scale includes psychosomatic items, as well as items reflecting feelings of tension, restlessness, etc. Seven items compose this scale, with a reproducibility of 87 percent.[4]

In order to employ dummy-variable regression analysis, interval measurement is assumed. However, in the present study and in the work of Treiman (1966) and Nelson (1968), ordinal data have been analyzed, albeit the results of such analysis must be considered tentative.

Rather than formulate a directional hypothesis, I believe that the present review suggests the adoption of the null hypothesis. Directionality may be observed from the distribution of scores. A reexamination of theory in the light of directionality of results will be more useful.

Results

In table 1, both the magnitude and the sign of the discrepancies between the observed and predicted mean scores are the bases for examining the

[4] The items of the scales are Community Integration (*a*) How often do you visit people living nearby? (*b*) Location of your friends (community or elsewhere)? (*c*) Do you think of present place of residence as your real home? (*d*) Length of residence in community (*e*) Are the people living in this community the same as you or different in important ways? *Primary Affiliation:* (*a*) *Have you made any* new friends in recent months? (*b*) How many good friends do you have? (*c*) How often did you get together with friends this past week? (*d*) How many different friends did you get together with this past week? Family Participation (*a*) Have you been in touch with relatives in the past two weeks? (*b*) How many relatives do you see regularly? (*c*) How many relatives were you in touch with? Psychosomatic Symptoms (*a*) In recent weeks have you been bothered by upset stomach? (*b*) Headaches? (*c*) Colds or flu? (*d*) General aches and pains? (*e*) Do you have trouble sleeping? Manifest Anxiety. (*a*) In the past few weeks have you been troubled by upset stomach? (*b*) Do you generally have enough energy? (*c*) Bothered by headaches? (*d*) Felt depressed? (*e*) Have trouble sleeping? (*f*) Feel nervous or tense? (*g*) So restless you can't sit still long?

TABLE 1

Observed Mean Scores and Discrepancies

Sons' SES	Fathers' SES: Observed Mean I	II	III	IV	Observed Minus Predicted Mean I	II	III	IV
				A Community Integration				
I	2.16 (29)	2.20 (45)	2.11 (41)	2.24 (50)	−0.05	−0.08	−0.16	−0.07
II	2.22 (15)	2.19 (38)	2.37 (37)	2.24 (56)	0.07	−0.03	0.16	−0.01
III	2.39 (4)	2.24 (32)	2.22 (56)	2.25 (57)	0.20	−0.02	0.03	−0.24
IV	2.67 (5)	2.06 (9)	2.33 (19)	2.28 (43)	0.48	−0.26	0.08	−0.01
				B Primary Affiliation				
I	1.17 (28)	1.18 (46)	1.14 (41)	1.12 (49)	−0.03	0.01	0.01	−0.04
II	1.20 (15)	1.15 (38)	1.11 (37)	1.05 (57)	0.04	0.02	0.02	−0.07
III	1.22 (4)	1.06 (33)	1.09 (55)	1.04 (58)	0.08	−0.05	0.02	−0.06
IV	1.22 (5)	1.14 (8)	1.10 (20)	1.06 (44)	0.12	0.07	0.07	0.00
				C Family Participation				
I	7.04 (24)	6.95 (41)	7.53 (38)	6.81 (43)	−0.17	−0.78	−0.10	−0.71
II	8.08 (13)	7.42 (33)	7.50 (28)	7.60 (48)	1.27	0.09	0.27	0.48
III	7.50 (2)	6.91 (32)	7.78 (46)	7.66 (47)	0.20	−0.91	0.08	0.05
IV	7.50 (4)	7.78 (8)	7.50 (16)	7.35 (31)	0.45	0.21	0.03	−0.01
				D Manifest Anxiety				
I	8.00 (29)	8.30 (46)	8.32 (41)	8.26 (50)	0.00	0.47	0.46	0.37
II	7.92 (16)	8.06 (38)	8.04 (36)	8.19 (57)	−0.30	0.01	−0.04	0.07
III	8.13 (4)	8.00 (34)	8.18 (56)	8.22 (60)	−0.21	−0.17	−0.02	−0.01
IV	7.30 (5)	7.76 (9)	8.47 (20)	8.25 (44)	−1.05	−0.42	0.26	0.01
				E Psychosomatic Symptoms				
I	0.97 (29)	0.95 (46)	1.04 (41)	1.05 (50)	0.03	0.00	0.04	0.08
II	0.97 (16)	0.96 (38)	1.03 (37)	1.06 (57)	0.02	0.00	0.02	0.08
III	1.03 (4)	1.03 (34)	1.08 (56)	1.12 (60)	0.00	0.01	−0.01	0.06
IV	0.77 (5)	0.95 (9)	1.12 (20)	1.09 (44)	−0.35	−0.11	0.01	0.01

Note.—N in parentheses

adequacy of the additive model.[5] Where the signs are negative, observed scores are lower than predicted; where the signs are positive, observed scores are higher than predicted. The mean scores of the stable sample are along the diagonal, from upper left to lower right. The cells above the diagonal represent the upwardly mobile; below the diagonal are the downwardly mobile cells.

One problem in drawing conclusions from the data in table 1 is the small cell sizes, especially in the three most downwardly mobile categories.[6] To summarize the overall pattern, table 2 was constructed by classifying the respondents as upwardly or downwardly mobile and stable. Weighted mean scores were computed for both observed and predicted values. These weighted means allow for probability estimates of the degree of discrepancy between observed and predicted means, and, following Treiman (1966), *t*-tests were applied.

Table 1, part A, shows that upwardly mobile respondents, in five of six cells, attained mean Community Integration scores lower than those predicted by the multiple-regression equation. Among the downwardly mobile, four of six cells have observed values higher than predicted. Table 2 reveals that overall, no differences between observed and predicted means attained significance at the .05 level.

The observed mean Primary Affiliation scores are generally higher than predicted among the downwardly mobile, with no apparent pattern among the upwardly mobile (table 1, part B). However, table 2 reveals an upwardly mobile score lower than predicted, and the difference between the predicted and observed scores is significant at the .01 level. None of the other summary means attained .05 significance level.

Family Participation scores in table 1, part C, reveal the downwardly mobile score generally higher than predicted by the multiple-regression equation. Among the upwardly mobile, no pattern is apparent. Summary mean scores shown in table 2 reveal that the down and up

[5] In addition to the statistical method reported here, a two-way analysis of variance was computed for each of the dependent variables, with current and prior SES as the independent variables In no case was the interaction effect significant at standard levels If the interaction terms had been significant, the multiplicative hypothesis would have received added support However, the failure to observe significant interaction effects does not negate the multiplicative hypothesis. The interaction term collects all the deviations, so that the test does not allow a test for patterns among specific groups Since we are basically interested in significant differences between observed and predicted means in particular groups of cells of table 1, nonsignificant results of the two-way analysis of variance do not negate the multiplicative hypothesis This problem is also encountered in Jackson and Burke (1965).

[6] The proportion of the present sample classified as up or down mobile cannot be compared with other nationally based studies. The suburban population in the present study serves the Washington, D C, area and contains a vastly disproportionate percentage of white-collar and upper-status occupations.

movers have lower scores than predicted. Among the upwardly mobile, the differences are significant, whereas among the downwardly mobiles, the lower than predicted observed mean score is not significant.

In summary, of the three measures of integration and/or participation in informal and intimate relationships, scores lower than predicted are observed among the upwardly mobile. None of the integration measures characterizes the downwardly mobile as friendless or lacking communal or familial contact. What is shown is a pattern of lower levels of participation in informal relationships. However, the multiplicative hypothesis receives support. Independent of class of origin and of destination, upward mobility appears to produce decreasing levels of participation in primary-like relationships. However, the data presented in table 1 reveal no systemic pattern associated with mobility.

Variation in levels of emotional adjustment may be seen in the data presented in table 1, parts D and E. Among the downwardly mobile there is an almost uniform pattern of scores lower than predicted, with the largest discrepancies appearing among the most downwardly mobile cells. By contrast, the upwardly mobile show a pattern of scores higher than predicted with respect to Manifest Anxiety Virtually no trends are apparent in the observed mean Psychosomatic Symptom scores. Examination of the summary scores in table 2 shows the *t*-test to be significant. The upwardly mobile have Manifest Anxiety scores higher than predicted,

TABLE 2

CUMULATED WEIGHTED OBSERVED AND PREDICTED MEAN SCORES

Mobility Experience	Observed Mean	N	Predicted	t
Community Integration				
Upward	2 23	286	2 27	1 54
Stable	2 22	166	2 25	1 24
Downward	2 27	84	2 23	0 88
Primary Affiliation				
Upward	1 10	288	1 13	2 99*
Stable	1 11	165	1 10	0 72
Downward	1 12	85	1 10	1 00
Family Participation				
Upward	7 34	245	7 48	1 67*
Stable	7 46	134	7 45	0 89
Downward	7 38	75	7 49	0 75
Manifest Anxiety				
Upward	8 23	290	8 01	3 15*
Stable	8 14	167	8 14	0 00
Downward	8 03	88	8 21	1 67*
Psychosomatic Symptoms				
Upward	1 05	291	1 00	2.91*
Stable	1 05	167	1 03	0 89
Downward	1 02	88	1 04	0 55

* Significant at $P \geq 05$ All others nonsignificant

while the downwardly mobile show scores lower than predicted. Similarly, the upwardly mobile display mean Psychosomatic Symptom scores higher than predicted. No other differences are significant. Once again, the multiplicative (nonadditive) hypothesis appears to be confirmed by these data.

The Effects of Extreme Mobility

In table 1 neither the magnitude of discrepancies nor the sign of the discrepant scores revealed any systematic pattern. Because of small cell frequencies, table 2 was constructed to test for overall patterns. Among the upwardly mobile, significantly lower scores on measures of participation and significantly higher scores on measures of emotional adjustment were observed. However, the results presented in table 2 obliterate the potential effects of the amount or degree of mobility. In order to test for the possible effects of variation in degree of mobility, table 3 was constructed. In table 3, the up and down movers were divided into those who had moved two or more status levels from their occupational origins and those

TABLE 3

CUMULATED WEIGHTED OBSERVED AND PREDICTED MEAN SCORE FOR DEGREE OF MOBILITY

Mobility Experience	Observed Mean	N	Predicted	t
Community Integration.				
Very up	2 20	147	2 27	2 02*
Moderately up	2 27	139	2.27	
Moderately down	2 26	66	2 23	0 57
Very down	2 31	18	2 23	0 81
Primary Affiliation				
Very up	1 10	147	1 14	3 00*
Moderately up	1 11	141	1 12	0 70
Moderately down	1 10	68	1 10	...
Very down	1.18	17	1.10	2 20*
Family Participation:				
Very up	7 32	129	7 40	0.71
Moderately up	7 37	116	7 56	1 47
Moderately down	7.31	61	7.51	1 18
Very down	7.43	14	7 38	0 02
Manifest Anxiety:				
Very up	8 25	148	7 97	3.31*
Moderately up	8 20	142	8 06	1 25
Moderately down	8 11	70	8 20	0 78
Very down	7.72	18	8.26	2.63*
Psychosomatic Symptoms·				
Very up	1 05	148	0.98	3.03*
Moderately up	1 04	143	1 01	1.16
Moderately down	1.04	70	1 07	0.76
Very down	0 92	18	0.92	...

* Significant at $P \geq 05$ All others nonsignificant

who had moved but one level. Again, predicted means were compared with observed means and the t-test was applied.

What emerges most distinctly from the results shown in table 3 is the pattern of mean observed scores among the very up mobile. In this group, the mean Community Integration score and the mean Primary Affiliation scores are significantly lower than the predicted means. This is not true, however, among the moderately up mobile group. Similarly, both the Manifest Anxiety mean and the Psychosomatic Symptom mean scores are higher and significantly different from predicted values, with no significant differences observed among the moderately up mobile.

Among the downwardly mobile, only one score attains significance, and it is lower than predicted. This occurs among the very down mobile on the Manifest Anxiety scale.

Implications and Comments

Through the use of dummy-variable multiple-regression analysis, levels that are lower than expected on Community Integration and Primary Affiliation were observed among the very up mobile. This same group displayed levels higher than predicted on Manifest Anxiety and Psychosomatic Symptoms.

Of course, no claim is made that the present study represents the "critical experiment" and that the recorded observations are the last word. Its major contribution rests on the use of a research design which is more adequate than previous studies have been. I have not taken into account the multiplicity of theoretical statements discussed earlier. My operational measure of status is based on an occupational indicator. The amount of movement which defines mobility is clearly arbitrary and is determined, in part, by the number of categories used to define present and prior SES. This procedure is justified in order to establish some common point of reference between this and previous research. If operational techniques were different, I could not adequately compare results.

I am testing for detrimental effects of mobility. My operations, while reflective of such global constructs as family disorganization, primary group or communal integration, and emotional adjustment, need to be understood as limited indicators of these more global concepts. I have observed levels of Family Participation lower than predicted among the upwardly mobile respondents. In more concrete terms, I have not recorded *family disorganization*. Rather, I have measured the frequency and number of familial interactions of the respondent. We can now recall Curtis's findings that frequency of family visits decreased with increased mobility. Adams also reported a lower frequency of weekly visits with parents among upwardly mobile white-collar men compared with stable blue-

collar men. It is fair to conclude that upward mobility is associated with lower levels of family contacts. It remains a moot question whether decreased contact reflects family disorganization. Clearly, Adams's findings of (1) few kin acquaintances, (2) low ratings of kin as "important in the scheme of things," and (3) less frequent reports of affectional closeness and value consensus with father among the *downwardly* mobile are factors more suggestive of familial disorganization. At the same time, the present study and Adams's study note relatively higher frequencies of contact with kin among the downwardly mobile.

A decrease in familial contacts associated with increasing upward mobility among men is a systematic finding. But we cannot move from these data to an inference about the quality of the relationships.

I do not intend to explain away the findings. But it is important to note what the findings do show. Equally important, we need to be continually alert to unsupported inference. Thus, I suggest that upward mobility is related to lower levels of family participation. I cannot characterize the analytic groups in antonymous terms as a result of this finding. The upwardly mobile cannot be described as "detached" or "isolated." The results show them to be only relatively "detached," as a comparison of the mean observed scores of the up, down, and stable groups reveals.

The two other integration measures—Primary Affiliation and Community Participation—are essentially limited to the frequency, variety, recency, and/or periodicity of such interaction We have no information as to the quality of these relationships. From these data, I consider it justified to conclude that participation in primary-like relationships tends to be lower among the upwardly mobile.

My data have also revealed, surprisingly perhaps in view of the earlier literature review, that the upwardly mobile have significantly higher levels of Manifest Anxiety and Psychosomatic Symptoms than expected Recall that the Midtown Manhattan studies observed the upwardly mobile to have the least risk of impairment and to have the highest proportions scoring in the "well" category. The more detailed data provided by Langner and Michael (1963) show that the more serious emotional disorders were characteristic of the downwardly mobile. The upwardly mobile were characterized as displaying higher rates of neurotic patterns. In the present study the two emotional adjustment scales reflect these less severe disturbances. No data were collected to test for alcoholism, "dyssociality," depression, rigidity, or schizophrenic reactions. All of the foregoing were observed to be statistically more frequent among the downwardly mobile in the Midtown Manhattan study. Hence, the findings of the present study confirm, in part, the Midtown Manhattan findings in that higher levels of *less serious* emotional disturbances are more characteristic of upward mobility.

The findings of the present study support a "modified" dissociative

hypothesis. Lower rates of interaction with family, friends, and community indicate differences in the degree of contact rather than an all-or-nothing view.

Thus far, I have confined the analysis and discussion to those findings which were statistically significant. Turning to table 3, we note that the downwardly mobile consistently attain the highest mean scores on the three integration measures. One score, Primary Affiliation, is significantly greater than expected. It is noteworthy that the literature review was able to report only on the studies of downward mobility and primary-group interaction by Adams and Curtis. Nevertheless, it has been assumed that downward mobility should reflect the dissociative pattern. Loss of status should make interaction stressful, in the strata both of origin and of arrival. The data recorded here do not support this hypothesis. Given the pattern of means higher than expected (although only one is significantly different), it would appear that downward mobility is related to moderately higher levels of interaction. Perhaps the downwardly mobile, who are declining in their position in the occupational structure, seek to compensate for loss of rank by increasing their participation in solidary groups. Alternatively, strong solidary ties may foster downward mobility. Robins, Gyman, and O'Neal (1962) have shown downward mobility to be a consequence of early behavior and values. If strong patterns of solidary relationships precede the opportunity for mobility, these strong ties may inhibit individual mobility.

In an earlier section of this paper, I discussed the position of Germani and myself that the disruptive effects of mobility were more likely to occur in a traditional society. More specifically, disruptive effects can be anticipated (1) if preparatory socialization was inadequate, (2) if mobility is relatively infrequent and/or when more than a single stratum is crossed, and (3) when movement is visible The present data, finding evidence of stress among those who moved up two or more strata, support the view that mobility, if it is to have disruptive effects, must "break the bounds," so to speak, of a stable system. While we have no direct evidence for inadequate preparatory socialization among the up mobiles, this inference has a face reasonableness. If these people are less than adequately prepared, their lower-status backgrounds may be visible to those situated above them. Further, movement over two or more strata is, statistically speaking, less frequent than movement from one to another. With these factors in mind, the evidence of higher levels of emotional disorder and lower levels of communal and affiliative interaction is supportive of Germani's position. For future researchers, we can anticipate personal and social disruption among current minority group members. For them, visibility and low rates of mobility will be part of their mobility experience.

The conditions listed above may be present among the very up mobile. However, Wilensky (1966) and Wilensky and Edwards (1959) present evidence showing work-life downward mobility (intragenerational) to be more significant than intergenerational mobility in shaping attitudes. The data presented here could be interpreted as the result of upward *intra*generational movement rather than disassociation due to intergenerational movement. Psychological stress and lower rates of solidary participation may be due to efforts to move up in the status hierarchy over and beyond that initial position which defines men as intergenerationally mobile.

A research design to test the comparative effects of inter- versus intragenerational mobility would obviously be quite elaborate. In his own work life, a man may be stable or an up or down mover. The same three categories apply to intergenerational movement, resulting in nine categories of men and no provision to account for degrees of movement. This design is not only elaborate, but would also require a very large sample.

I believe this study permits us to assert the existence of demonstrable empirical consequences of mobility. I have considered an alternative interpretation as to the "causes" of the observed patterns. These alternatives remain to be tested. Without the test, we have confidence in empirical results but not in the theory of causality.

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Mobility and Work Satisfaction: A Discussion of the Use and Interpretation of Mobility Models[1]

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Two different models for studying the effects of intergenerational occupational mobility on three types of work satisfaction are compared and discussed. The interpretive consequences of using one model rather than another are considered. The analysis shows that mobility, whether studied by the interaction or additive models, is not significantly related to satisfaction with earnings. The additive model can predict a significant relationship between mobility and satisfaction with the kind of work done and overall work satisfaction and is preferable to the interaction model on grounds of simplicity.

In this paper, the interpretive implications of using an interaction compared to an additive model to study the effects of social mobility on three types of work satisfaction will be discussed. Blalock (1966) and others have focused on the statistical properties and problems of using each of these models. The purpose here is not to present a review of the literature on mobility but to discuss the ways in which interpretations associated with the effects of social mobility are related to the statistical model used. It will be argued that the interpretation of a set of findings must take account of the varying characteristics of different statistical models.

The general argument is as follows: social mobility as a variable should, as is true for any variable, be used to increase precision in the explanation of some observed relationship in empirical phenomena. Using the variable "social mobility" postulates some special (interaction) effect resulting from a comparison between one's original and one's present status. It involves the claim that adding the characteristic of movement or stability in social status across a period of time explains variation in outcome beyond what is explained by looking at the relationship of father's occupation and son's occupation to a dependent variable separately. Thus, social mobility as an independent variable implies the greater force of an interaction compared to an additive model in explaining a particular

[1] I am particularly indebted to Robert W. Hodge for his guidance and encouragement, and to the comments and criticisms of Phillip Bonacich, Melvin Seeman, and Andrea Tyree. The data used in this analysis were made available by Norman M Bradburn and the National Opinion Research Center, for which appreciation is expressed.

behavior or attitude. Before this model is adopted, though, the researcher must demonstrate that the more complex interaction model explains variation in a dependent variable to a significantly greater extent than does the more parsimonious additive one.

SOME MEANINGS OF SOCIAL MOBILITY

What do we mean by mobility? Usually, we are referring to movement between categories that we have imposed on some set of observations or some available data over a period of time. Thus, we may learn that Mr. *X* is now engaged in an occupation. We may also learn that Mr. *X*'s father was engaged in an occupation which may be the same as or different from that occupation engaged in by Mr. *X*. If Mr. *X* works in an occupation that is different from that of his father, and if the difference is such that it will become apparent according to the classification system imposed on the data, we then say that mobility has occurred. If Mr. *X* is in the same classificatory category as his father, we say that there has been no mobility or that there has been intergenerational occupational stability.[2] Looking at the effects of movement between occupational categories with a constructed variable (social or occupational mobility) rather than the components of which that variable is composed (father's occupation and son's occupation) implies that there is some special value associated with movement or stability which explains behavior above and beyond what is explained by taking account of each element independently.

The distinction between an emphasis on "process" in contrast to an emphasis on independent parts is rarely made explicit in the social and occupational mobility literature. On the contrary, there is, most often, an automatic concentration on "process," on change, since this is the very meaning of the mobility variable. Using a "manufactured" variable such as social or occupational mobility *assumes* the importance of these processes without testing the validity of this assumption. Nevertheless, the distinction between process and component parts has important conceptual and interpretive consequences.[3]

Let us look at the formal structure of the argument and its associated interpretations. To analyze the effect of intergenerational occupational mobility on some set of attitudes, a minimum of three pieces of information is required: (1) father's occupation, (2) respondent's occupation, and (3) some reliably measured dependent variable. An expression of the

[2] Differences between *social* and *occupational* mobility are neither ignored nor considered unimportant. In the context of the present formulation, social mobility refers to intergenerational occupational mobility *only*

[3] This distinction is discussed in Hodge and Treiman (1966).

effect of the two independent variables on the dependent variable can take the form of

$$Y_i = a + bX_i + cZ_i + e_i, \quad (1)$$

where Y_i represents the dependent variable, X_i represents the respondent's occupation, Z_i represents the father's occupation, and e_i is a random variable with mean zero.

The same three variables can be put together in yet another way which has important consequences for the meaning of the argument involved. This construction would take the form

$$Y_i = a' + b'X_i + c'Z_i + d'X_iZ_i + e'_i, \quad (2)$$

where X, Y, and Z represent the same variables as in equation (1), and e'_i again is a random variable with mean zero.[4]

To the extent that the variables X and Z significantly explain variations in Y using equation (1), the interpretations which are associated with the analysis must be restricted to the meaning of each separate independent variable. There is nothing in the equation which says that the interpretation associated with one independent variable should be modified to take account of the value of the other independent variable which appears in the same equation.

In considering results achieved using an additive model to study work satisfaction, it might be suggested that father's occupation located the respondent, as part of his family of origin, in social space, which affected some of the respondent's early experiences. We also know that a father's earnings are related to his son's occupation choice (Rosenberg 1957, pp. 53–61) and that father's occupation is related to a son's position and participation in the labor market (Blau and Duncan 1967). It also is likely that the son has learned certain attitudes, behavior, and orientations from his father (Dyer 1956) which may be manifested in the adult attitudes and behavior which a respondent displays.

A father's occupation may, therefore, have a structural impact on his

[4] Model 2, which takes account of interaction effects, might also be written as

$$Y_i = a' + b'X_i + c'Z_i + d'|X_i - Z_i| + e'_i, \text{ or}$$
$$Y_i = a' + b'X_i + c'Z_i + d'(X_i/Z_i) + e'_i,$$

where X, Y, and Z represent the same variables as in the formulations presented in the body of the text, and the e'_i's again are random variables with mean zero. The various forms of the "mobility" variable, that is, X_iZ_i, $|X_i - Z_i|$, and (X_i/Z_i) reflect alternative formulations of the effect of mobility. Dummy variables were used in the analysis presented in this paper. The model included in the text is, of the three alternatives presented, the only one appropriate to that procedure. Measuring mobility by the absolute difference or by a ratio assumes continuous variables. This assumption is avoided by the use of dummary variables.

son's behavior in his work world in terms of position as well as affecting his attitudes toward work. The father's experiences and attitudes toward his own work may become part of his son's "data bank" and be called forth under certain conditions and at certain times in the son's adult life. The relation of a respondent's own occupation to the satisfactions he may feel with his work undoubtedly has numerous referents in the structure of demands, rewards, and expectations which an individual brings to and finds in his own work situation. Some aspects of this structure are discussed elsewhere (see Laslett 1969).

If one is going to work from the model presented in equation (2), then a discussion of change and comparison is both required and justified. The presence of the interaction term in the equation necessitates that attention be paid to this process. The question, then, is whether the variation observed in the dependent variable can be explained independently by the component parts or whether adding an interaction term to them significantly increases our understanding. If it does not, the additive model must be accepted on grounds of simplicity and the interpretation of findings must be associated with the independent terms of which the model is composed, without any comparison between them.

In relation to the present case, that of the association between intergenerational occupational mobility and work satisfaction, the argument about the effects of mobility has in most cases been phrased in terms of reference group activities and behavior. This concept of reference groups suggests that individuals evaluate their behavior and achievements by comparing them with specific and identifiable groups which, in the context of mobility, means the group of origin and the group of destination. Such a formulation contains at least two assumptions: (1) individuals asked to make this evaluation have a minimum of two reference groups in mind, current and previous membership groups; and (2) a comparison of these two *specific* groups is made to reach the evaluation. Does the evidence we have on the relationship between intergenerational occupational mobility and work satisfaction support the validity of accepting these assumptions which are implicit in the meaning of the relationship?

SOCIAL MOBILITY AND WORK SATISFACTION

Form and Geschwender (1962), in their study of the effects of differing reference groups on the job satisfaction of manual workers, propose "that sub-groups in a society respond differently to the occupational structure, that their evaluations of their occupational status is a response to their social and mobility experiences, and that changes in their objective positions condition their job evaluations" (p. 228). Thus, what they attempt to show is that "for manual workers in an industrial community, job

evaluations are . . . a response to the occupation which their parents and brothers occupy" (p. 229). Their findings indicate that upwardly mobile sons have significantly higher job satisfaction scores than do downwardly mobile sons. The satisfaction scores of intergenerationally stable sons are somewhat lower than upwardly mobile sons and significantly higher than the scores of downwardly mobile sons.[5]

Bonjean, Bruce, and Williams (1967), doing a replication of Form and Geschwender's research but extending the sample to include managers as well as manual workers, found that there was a positive, though not statistically significant, association between job satisfaction and generational mobility among manual workers, but that this was not true for the managers in their sample. Bonjean et al. (1967) explain their findings in terms of the greater salience of "family" among manual than managerial workers. In effect, they are interpreting their findings by referring to the respondent's current occupational status: whether it is manual or non-manual. They say that a "reference group interpretation of job satisfaction would (not) be inappropriate for middle class persons, only that, if applicable, such persons use different reference points" (p. 501).

Tausky and Dubin (1965) discuss somewhat similar issues in a study of middle-level management personnel in industry by using their notion of "career anchorage," meaning the reference point to which occupational aspirations are attached They say, "Some individuals value top-level positions highly and strive for them throughout their occupational lives, while others value the occupational progress already experienced" (p. 726). Translating this concept into reference group language, some men will use their previous social status or position of origin as the standard by which they may judge their own occupational accomplishments and satisfactions. Other men may use a group of which they are not yet members but to which they aspire for membership as a reference point.

Implicit throughout this formulation is the assumption that movement, that is, mobility (although here we are looking at it intragenerationally not intergenerationally), has some special effect but that this effect varies according to the criteria used to evaluate it. If men focus on the amount of movement already experienced from their point of origin, they will evaluate their careers differently than if they focus on the top of the occupational hierarchy of which they are a part and which they wish to reach. Tausky and Dubin (1965, p. 730) find that career anchorage is one

[5] It should be noted that Form and Geschwender measure mobility by a ratio rather than the multiplicative interaction term emphasized in this presentation The focus of this discussion is a comparison between the meanings implicit in additive and interaction models and not on the differences in meaning which might be attributed to alternative ways of formulating the interaction term This argument is not affected by the difference between Form and Geschwender's representation of mobility and the one used here.

factor in work satisfaction. Of the upwardly anchored men, 78 percent claim they would be dissatisfied if they never reached the top executive level, while 44 percent of the downwardly anchored men make this evaluation. While Bonjean et al. say we do not know the appropriate reference group which managerial employees use to make their job satisfaction evaluations, Tausky and Dubin are suggesting that there may be not one but several groups involved in this process. Whatever the number or complexity of the reference groups involved in the evaluation procedure, though, emphasis is placed on the importance of movement and on comparison between present and past or future status which is assumed to accompany it.

ANALYSIS

The present analysis examines the effects of intergenerational occupational mobility on work satisfaction among men in a wider range of occupations and covering a wider geographical area than has been usual in past research. Furthermore, the analysis looks at the effect of such mobility on three different types of work satisfaction: satisfaction with earnings, satisfaction with the kind of work done, and general or overall work satisfaction.

The data on which this analysis is based comes from a survey conducted by the National Opinion Research Center (NORC) during 1962–63 (see Caplovitz and Bradburn 1965; Bradburn 1969). The total sample consists of 2,787 males and females in four communities (Warren Township, Michigan; Inner City Detroit; Northwest Chicago; and a Washington, D.C., suburban county) and the ten largest metropolitan areas in the country. A random probability sample was drawn from each of these areas. The present analysis will be restricted to the male respondents ($N = 1,256$)[6] who were members of the labor force—both employed and unemployed—at the time of the interview.[7] Although each sample was chosen randomly from the community of which it was a part, the total sample is not representative of any one universe. In consequence,

[6] The subsamples are distributed in the following way Washington suburban sample, $N = 594$; Inner City Detroit, $N = 168$; Warren Township, Michigan, $N = 248$; Northwest Chicago, $N = 131$; ten metropolitan areas, $N = 115$

[7] The decision to exclude the female sample from the present analysis was made on several grounds: (1) Hulin and Smith (1964) have shown that evaluation of the rewards from work are different for men and women; (2) the reasons for which women decide to work vary, and this variance is likely to influence their perception of rewards (this is less likely to be true of men whose social role imposes the expectation that they will work); and (3) Oppenheimer (1970) shows that work opportunities for women in the labor market differ from those of men. Furthermore, the work satisfaction questions were asked in relation to the chief household wage earners, only a small proportion of which were women

conclusions can at best be suggestive of the relationships which may be found in a more representative population.

As already mentioned, three measures of work satisfaction were used in the present analysis: satisfaction with earnings, satisfaction with the kind of work done, and an overall general evaluation of work satisfaction. In the context of a series of questions relating to the respondent's work situation and experiences, the following questions were asked: How satisfied are you with your earnings? How satisfied are you with the kind of work you do? Taking all things together, how do you feel about your work as a whole? The possible responses to each of these questions were: very satisfied, somewhat satisfied, somewhat dissatisfied, and very dissatisfied.[8]

The 1950 census categories were used for coding the NORC occupational data. This system of classification has various shortcomings. Each broad class of occupations tends to combine different jobs and statuses into one category (see Hodge and Siegel 1966).[9] Using these classificatory categories, though, does permit comparisons to be made with previous research in which the same occupational groupings as well as the blue-collar–white-collar dichotomy have been involved.

Mean satisfaction with earnings scores are presented in table 1 by father's and respondent's occupation. The satisfaction variables were scored from "very dissatisfied" (1) to "very satisfied" (4). No "neutral" response category was available to respondents. In order to generate both the actual scores (presented in panel 1) and scores that would be expected on the basis of an additive model (presented in panel 2), the occupa-

[8] Some question may be raised as to the respondent's ability to distinguish these three different types of satisfaction and to respond to each separately. There are, as one would expect, high, although far from perfect, correlations between the three types of satisfaction (X_1 = satisfaction with earnings, X_2 = satisfaction with work done, X_3 = overall satisfaction with work)

	X_1	X_2
X_2	0.309	
X_3	0.362	0.595

The relationship of intergenerational occupational mobility to each type of satisfaction is similar—although there are some differences—and the general line of the argument that applies to one applies to all. But it was considered of value to report the findings for the three types of satisfaction, since each would provide a reliability check for another as well as showing that the argument holds for different types of work satisfaction

[9] In addition to the deficiencies of this system of classification, it has been necessary to collapse these categories still further in order to have a sufficient number of cases in each group. Although such a procedure may have some effect on the amount of mobility observed, the data permit no alternative.

TABLE 1

ACTUAL AND EXPECTED MEAN SATISFACTION WITH EARNINGS SCORES IN RELATION TO RESPONDENT'S OCCUPATION AND FATHER'S OCCUPATION

Father's Occupation	Respondent's Occupation: Upper White Collar	Lower White Collar	Upper Blue Collar	Lower Blue Collar	Total
	1. Actual Mean Satisfaction with Earnings Scores				
Upper white collar	2.92	2.88	3.11	3.28	3.01
Lower white collar	3.11	2.78	3.26	3.14	3.07
Upper blue collar	2.99	2.94	3.01	2.95	2.98
Lower blue collar	3.01	2.80	3.08	2.93	2.97
Farm	3.29	2.75	3.13	3.04	3.09
Total	3.01	2.84	3.09	3.01	...
	2. Expected Mean Satisfaction with Earnings Scores				
Upper white collar	3.02	2.84	3.08	3.00	...
Lower white collar	3.10	2.92	3.17	3.08	...
Upper blue collar	2.99	2.81	3.05	2.97	...
Lower blue collar	2.99	2.80	3.05	2.97	...
Farm	3.09	2.91	3.15	3.07	...
	3. Actual Minus Expected				
Upper white collar	−0.10	0.04	0.03	0.28	...
Lower white collar	0.01	−0.14	0.09	0.06	...
Upper blue collar	0.00	0.13	−0.04	−0.02	...
Lower blue collar	0.02	0.00	0.03	−0.04	...
Farm	0.20	−0.16	−0.02	−0.03	...
	4. Number of Cases				
Upper white collar	100	28	45	29	202
Lower white collar	27	18	19	7	71
Upper blue collar	102	34	92	54	282
Lower blue collar	86	45	80	106	317
Farm	34	16	64	85	199
Total	349	141	300	281	1,071

tional categories presented in table 1 were represented as dummy variables and the work satisfaction scores were regressed upon them (Suits 1957).[10] Expected scores were obtained by adding the coefficients for each individual's origins and destination to the grand mean (see tables 4–6). Thus, for a respondent whose father was an upper-white-collar worker and who is himself a lower-white-collar worker,

$$\begin{aligned}\hat{S}_i &= a + b_2 + c_1. \\ &= 3.069 + (-.163) + (-.069). \\ &= 2.84.\end{aligned}$$

[10] This methodological description applies to the derivation of scores for all three types of work satisfaction.

TABLE 2

ACTUAL AND EXPECTED MEAN SATISFACTION WITH KIND OF WORK DONE SCORES IN RELATION TO RESPONDENT'S OCCUPATION AND FATHER'S OCCUPATION

FATHER'S OCCUPATION	RESPONDENT'S OCCUPATION: Upper White Collar	Lower White Collar	Upper Blue Collar	Lower Blue Collar	Total
	1 Actual Mean Satisfaction with Work Done Scores				
Upper white collar	3.53	3.15	3.62	3.24	3.46
Lower white collar	3.44	3.47	3.53	3.43	3.47
Upper blue collar	3.55	3.33	3.48	3.32	3.46
Lower blue collar	3.54	3.36	3.52	3.32	3.44
Farm	3.67	2.81	3.53	3.28	3.39
Total	3.55	3.26	3.53	3.30	
	2 Expected Mean Satisfaction with Work Done Scores				
Upper white collar	3.53	3.25	3.52	3.30	.
Lower white collar	3.57	3.29	3.55	3.33	.
Upper blue collar	3.54	3.26	3.52	3.30	
Lower blue collar	3.56	3.28	3.54	3.32	.
Farm	3.52	3.24	3.50	3.28	.
	3 Actual Minus Expected				
Upper white collar	0.00	−0.10	0.10	−0.06	
Lower white collar	−0.13	0.18	−0.02	0.10	
Upper blue collar	0.01	0.07	−0.04	0.02	...
Lower blue collar	−0.02	0.08	−0.02	0.00	.
Farm	0.15	−0.43	0.03	0.00	
	4 Number of Cases				
Upper white collar	98	27	45	29	199
Lower white collar	25	17	19	7	68
Upper blue collar	100	33	92	53	278
Lower blue collar	86	44	78	100	308
Farm	33	16	64	81	194
Total	342	137	298	270	1,047

Mean scores for satisfaction with work done and overall work satisfaction are presented in tables 2 and 3, respectively. The equation for calculating the expected scores is of the form:

$$\hat{S} = a + \sum_{i=1}^{4} b_i X_i + \sum_{j=1}^{5} c_j Z_j, \qquad (3)$$

where $\hat{S}$ = the work satisfaction score; $X_1 = 1$ if the respondent works in an upper-white-collar occupation and 0 otherwise; $X_2 = 1$ if the respondent is in a lower-white-collar occupation and 0 otherwise; $X_3 = 1$ if the respondent is in an upper-blue-collar occupation and 0 otherwise; $X_4 = 1$

TABLE 3

ACTUAL AND EXPECTED MEAN GLOBAL (OVERALL) WORK SATISFACTION SCORES IN RELATION TO RESPONDENT'S OCCUPATION AND FATHER'S OCCUPATION

	RESPONDENT'S OCCUPATION				
FATHER'S OCCUPATION	Upper White Collar	Lower White Collar	Upper Blue Collar	Lower Blue Collar	Total
	1 Actual Mean Global (Overall) Work Satisfaction Scores				
Upper white collar	3.49	3.19	3.53	3.31	3.43
Lower white collar	3.68	3.59	3.47	3.14	3.54
Upper blue collar	3.51	3.45	3.42	3.15	3.40
Lower blue collar	3.42	3.36	3.44	3.39	3.41
Farm	3.55	3.00	3.50	3.36	3.41
Total	3.50	3.34	3.46	3.32	..
	2 Expected Mean Global (Overall) Work Satisfaction Scores				
Upper white collar	3.49	3.32	3.46	3.31	..
Lower white collar	3.61	3.44	3.58	3.44	...
Upper blue collar	3.47	3.30	3.44	3.29	..
Lower blue collar	3.50	3.33	3.47	3.32	
Farm	3.51	3.34	3.47	3.33	...
	3 Actual Minus Expected				
Upper white collar	0.00	−0.13	0.07	0.00	
Lower white collar	0.07	0.15	−0.11	−0.30	
Upper blue collar	0.04	0.15	−0.02	−0.14	.
Lower blue collar	−0.08	0.03	−0.03	0.07	...
Farm	0.04	−0.34	0.03	0.03	...
	4 Number of Cases				
Upper white collar	98	27	45	29	199
Lower white collar	25	17	19	7	68
Upper blue collar	100	33	92	53	278
Lower blue collar	86	44	78	100	308
Farm	33	16	64	81	194
Total	342	137	298	270	1,047

if the respondent is in a lower-blue-collar occupation and 0 otherwise.[11] Each $Z_j = 1$ if the respondent's father was in a similarly defined occupational category and 0 otherwise, with the addition of one further category, $Z_5 = 1$, if the respondent's father was in farming and 0 otherwise. The values for the expected minus actual means are presented in panel 3 of the tables.

[11] The upper-white-collar group includes the Census categories of professional, technical, and kindred workers, and managers, officials, and proprietors; the lower-white-collar group includes the Census categories of clerical, sales, and kindred workers; the upper-blue-collar group includes the Census categories of craftsmen, foremen, and kindred workers and the lower-blue-collar group includes the Census categories of operatives and kindred workers, service workers, and laborers, except those in farming and mining.

TABLE 4

NET REGRESSION COEFFICIENTS AND $\hat{S}$ OBTAINED BY REGRESSING FATHER'S AND SON'S OCCUPATION ON SATISFACTION WITH EARNINGS

Son's Occupation	Net Regression Coefficient	Father's Occupation	Net Regression Coefficient
Upper white collar ..	$b_1 = 0.019$	Upper white collar ...	$c_1 = -0.069$
Lower white collar ..	$b_2 = -0.163$	Lower white collar .	$c_2 = 0.014$
Upper blue collar	$b_3 = 0.083$	Upper blue collar . .	$c_3 = -0.101$
Lower blue collar .	$b_4 = 0.016$	Lower blue collar .	$c_4 = -0.103$
		Farm	$c_5 = 0.049$

NOTE — $a = \bar{S} = 3.069$

The solution of regression equation (3) where $\hat{S}$ refers to satisfaction with earnings provides the dummy variable net regression coefficients shown in table 4. The solution of the regression equation where $\hat{S}$ refers to satisfaction with the kind of work done provides the net regression coefficients shown in table 5. For the solution of the same equation where $\hat{S}$ refers to overall (general) work satisfaction, see table 6 for the net regression coefficients.

TABLE 5

NET REGRESSION COEFFICIENTS AND $\hat{S}$ OBTAINED BY REGRESSING FATHER'S AND SON'S OCCUPATION ON SATISFACTION WITH KIND OF WORK DONE

Son's Occupation	Net Regression Coefficient	Father's Occupation	Net Regression Coefficient
Upper white collar	$b_1 = 0.237$	Upper white collar ..	$c_1 = 0.015$
Lower white collar	$b_2 = -0.046$	Lower white collar . .	$c_2 = 0.051$
Upper blue collar	$b_3 = 0.221$	Upper blue collar ...	$c_3 = 0.020$
Lower blue collar . .	$b_4 = -0.106$	Lower blue collar	$c_4 = 0.039$
		Farm	$c_5 = -0.023$

NOTE —$a = \bar{S} = 3.282$

TABLE 6

NET REGRESSION COEFFICIENTS AND $\hat{S}$ OBTAINED BY REGRESSING FATHER'S AND SON'S OCCUPATION ON GENERAL (OVERALL) WORK SATISFACTION

Son's Occupation	Net Regression Coefficient	Father's Occupation	Net Regression Coefficient
Upper white collar ..	$b_1 = 0.178$	Upper white collar .	$c_1 = -0.018$
Lower white collar ...	$b_2 = 0.010$	Lower white collar .	$c_2 = 0.108$
Upper blue collar ...	$b_3 = 0.146$	Upper blue collar . . .	$c_3 = -0.037$
Lower blue collar	$b_4 = -0.086$	Lower blue collar ...	$c_4 = 0.008$
		Farm	$c_5 = 0.008$

NOTE $a = \bar{S} = 3.328$

Looking at the regression slopes which are associated with the different satisfaction variables, no clear pattern is apparent. Although it has frequently been asserted that the higher an individual is placed in the occupational hierarchy the more likely he is to be satisfied with his occupation (see, e.g., Blauner 1960; Inkeles 1960), the present analysis does not support this conclusion. Inspection of the data used to support this past conclusion, however, shows great variety in sample sizes and compositions of the different studies used as evidence.[12] Furthermore, there has been considerable variation in the wording of questions used to measure work satisfaction, and in some instances measurement of a different attitude is used in lieu of a direct work satisfaction query. For instance, Morse and Weiss (1955) asked their respondents whether or not they would continue in the same type of work if they inherited enough money to live comfortably without working. Blauner (1960) used the responses to this question as an index of work satisfaction. In the Fortune-Roper Survey (1947), a question which refers to how interesting or monotonous a worker considers his job is used by Blauner (1960) as evidence for the relationship he asserts between occupational ranking and job satisfaction. Yet *interest* is not necessarily equivalent to *satisfaction*. If, as Goldthorpe (1966, p. 229) suggests, some workers view their jobs in instrumental terms such that "work was for them primarily a means to ends external to the work situation," then, if these ends were achieved, the extent of interest which the job held for the worker would not necessarily affect his feeling of satisfaction. The assertion that there is a positive relationship between occupational level and work satisfaction is therefore not as conclusive as some authors have suggested.

Looking again at the regression slopes associated with the occupational categories for the three types of work satisfaction (tables 4, 5, 6), we can observe that among the sons—that is, the respondents—there is a positive relationship between occupational level and satisfaction *within* the blue- and white-collar categories: the higher the occupation, the greater the average work satisfaction. This pattern was discussed by Thomas (1956) in his review of the literature on work satisfaction and is supported by the present analysis. Satisfaction scores decrease as one

[12] Of the seventeen studies which Herzberg et al (1957, pp 20–21) mention as evidence for the "unequivocal fact" that "the higher the level of occupation, the higher the morale," the study by Katz refers to the employees of a large insurance firm; Mann refers to the nonsupervisory employees of a light and power company; Ahlberg and Honey report on chemists, engineers, and physicists, Kates refers to policemen; and Ash reports on workers in a steel plant. Only Centers and the Fortune-Roper studies report on findings from national samples in which the same question was asked of people in many different occupations Herzberg does not specify the other nine studies which make up the seventeen that support his statement.

moves from upper- to lower-level positions within the white- and blue-collar occupational groupings, but this pattern does not describe the entire occupational hierarchy.

The observed relationship between position within the white- and blue-collar occupational groupings and work satisfaction does not hold up, however, when we look at the effect of father's occupation on the respondent's evaluation of his work satisfaction. If a father's occupation is not related to the satisfaction which his son derives from his work, why should we think that father's occupational achievement operates as a reference group criteria or that a comparison between the son's and father's occupational affiliation is relevant to the son's satisfaction evaluation?

TABLE 7

ACTUAL AND EXPECTED MEAN SCORES FOR THREE TYPES OF WORK SATISFACTION, IN RELATION TO INTERGENERATIONAL OCCUPATIONAL MOBILITY

Mobility Experience	Mean Satisfaction Scores: Actual	Expected	Actual-- Expected	N
	Satisfaction with Earnings			
Upward	3 00	2 97	0 03	(374)
Stable	2 94	3 01	—0 07	(316)
Downward ..	3 07	3 01	0 06	(182)
Farm origins .	3 09	3 09	0 00	(199)
Total ..	..	..	.	(1,071)
	Satisfaction with Work Done			
Upward	3.49	3 49	0 00	(366)
Stable	3 44	3 45	—0 01	(307)
Downward . . .	3 38	3.38	0 00	(180)
Farm origins ..	3 39	3 39	0 00	(194)
Total	. .	..	..	(1,047)
	Overall Work Satisfaction			
Upward	3 46	3 45	0 01	(366)
Stable	3.44	3 42	0 02	(307)
Downward ...	3 31	3 38	—0 07	(180)
Farm origins .. .	3 41	3.41	0 00	(194)
Total	.	..	...	(1,047)

Table 7 summarizes the actual-minus-expected discrepancies presented in tables 1–3 with respect to occupational mobility. The cell entries were generated by taking a weighted average of the appropriate values in each of the first three panels of the original tables. The values in the cells to the right of the diagonal in any panel constitute the upwardly mobile

category. The cells on the diagonal comprise the stable category. The downwardly mobile category is composed of a weighted average of the values to the left of the diagonal, excluding the values for those respondents with farm origins. The mean value for this last category was computed separately

Although we can see in table 7 that predictions based on an additive model are in seven out of twelve cases different from those actually observed in the data, there are no appreciable differences in any of these values, and, more important, there is no consistent pattern to these discrepancies. The additive model does not produce consistently higher or lower values than does the interaction model which provided the actual values.

When we look at the three sets of actual work satisfaction scores entered into the table, we see that the mean satisfaction with earnings score is likely to be lower than either of the other two work satisfaction items. Yet, the highest mean satisfaction with earnings score is observed among people who have been downwardly mobile. Within each of the other two satisfaction groups, there is a positive relationship between the type of mobility experience and satisfaction, if one excludes the "Farm Origins" category.

The differences between the actual (or observed) scores generated by the interaction model and scores expected on the basis of an additive model are small and inconsistent Are they statistically significant? Table 8 presents the multiple correlations associated with different efforts to estimate the relationship between intergenerational occupational mobility and work satisfaction scores using additive and interaction models. The F-test was used to test the statistical significance of the difference between estimates generated by the two models. Two types of interaction models were used: one added mobility terms only into the equation, and the other included all cross-products terms.[13]

Although using an interaction model to estimate the relationship of mobility to each type of work satisfaction increases the correlation between the dependent and independent variables, in only one comparison is the difference significant. It must be noted, though, that the one significant difference is not between the additive and mobility models but between the additive and general interaction model. Although there may be statistically significant interactions in these data, they are not those which would be predicted on a theory of the effect of intergenerational mobility on work satisfaction. Intergenerationally upward, downward, or stable respondents do not have consistently high or low work satisfaction scores

[13] M_1 = upward mobility = $X_i Z_j$, where $i > j$; M_2 = stability = $X_i Z_j$, where $i = j$; M_3 = downward mobility = $X_i Z_j$, where $i < j$

TABLE 8

SUMMARY OF REGRESSION ANALYSIS OF ADDITIVE VERSUS INTERACTION MODELS[a]

Type of Work Satisfaction	Multiple Correlations: Additive Model (A)	Integration Models: Mobility (B)	Integration Models: All Cross-Products (C)	F-Test[b] for Significance of Difference between Models	N
Satisfaction with earnings ...	0 103	0 117	0 141	A vs B = n s	(1070)
				A vs C = 8 04*	(1070)
				B vs C = n.s	(1070)
Satisfaction with kind of work done	0 157	0 157	0 187	A vs B = n s	(1047)
				A vs C = n.s	(1047)
				B vs C = n.s	(1047)
Overall work satisfaction ...	0 119	0 136	0 160	A vs B = n.s	(1047)
				A vs C = n s	(1047)
				B vs C = n s	(1047)

[a] Explaining the relationship of respondent's occupation, respondent's father's occupation, and three different types of work satisfaction

[b] The equation used to compute the F presented in this table is

$$F = \frac{R_I^2 - R_A^2}{1 - R_I^2} \cdot \frac{N - m_2 - 1}{m_2 - m_1},$$

where R_I^2 = the amount of variance explained by the interaction (or mobility) model, R_A^2 = the amount of variance explained by the additive model, N = the number of observations, m_1 = the number of dummy variables in the additive model, and m_2 = the number of variables in the interaction or mobility model (NORC 1967, p 76)

* Significant at .01 level

which can be attributed to the effects of mobility. Thus the notion that intergenerational occupational mobility effects work satisfaction must be rejected.

The logic of the argument as presented here would lead to accepting the additive model as one consequence of rejecting the interaction model. In the present case, however, both models must be rejected in relation to satisfaction with earnings, since neither model produces a statistically significant estimate. The additive model does provide a significant estimate of the relationship between intergenerational occupational mobility and satisfaction with work done ($P < .01$) and overall work satisfaction ($P < .05$).[14]

CONCLUSION

In this paper two different models for studying the effects of social mobility on work satisfaction have been compared, the additive and interaction

[14] Father's occupation does not significantly effect the respondent's work evaluation, so we are really speaking about the effect of the respondent's occupation only. While occupation is significantly related to both satisfaction with work done and overall satisfaction on the level of gross effects, it was found to be significantly related to the former satisfaction only when the net effects were analyzed (see Laslett 1969)

models. The interpretive consequences and limitations of using one model rather than another have also been discussed.

Since the interaction model does not add significantly to our understanding of the effect of social mobility on work satisfaction, it must, at least temporarily, be rejected. The additive model, to which one would turn on the grounds of simplicity, also explains little of the variance in satisfaction with earnings but does explain a significant (though small) amount of the variance in satisfaction with work done and overall work satisfaction. This refers to the effect of son's occupation only, since father's occupation is not significantly related to any of the satisfaction items. When this is the case, there seems little support for the idea that father's occupation serves as a reference group for the son in evaluating his own occupational satisfaction.

The interpretive implications of using an additive model are constrained by having to view each component independent variable separately. Thus, the effect of the respondent's occupation upon his work satisfaction can be discussed, but the interpretation may not include a comparison between the father's and son's occupation. Only when the interaction model explains a statistically significant amount of the variance beyond what is explained when an additive model is used can a comparison between father's and son's occupation be included in the interpretation.

Once the appropriate statistical model has been determined, and the interpretation of findings has taken into account the constraints and conditions which that model imposes, there are further questions to be investigated. What are the sociological, compared to statistical, reasons for using one set of variables to explain variation rather than another? One answer to this question may be sought in the structural bases of individual lives. The varying structural arrangements in which individuals are embedded may call forth, or make relatively more salient for some period of time, certain aspects of a person's experience, past or present. A particular social context may engage one set of values, events, ideas. A particular time period of one's life may engage another. Attention to structural variation and structural differentiation may provide a basis for understanding the ways in which judgments and evaluations are made.

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Status Inconsistency and Ethnoreligious Group Membership as Determinants of Social Participation and Political Attitudes[1]

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As a test of the theory of status inconsistency, the effects of ethnoreligious group membership and education on several economic and political attitudes and indicators of social participation were analyzed, using survey data on a white native-born Detroit sample It was hypothesized that if status-inconsistency effects were operating, we would observe statistical interaction effects between ethnoreligious group membership and education, and that these interactions would vary as a function of ethnoreligious status. For some variables and for some ethnoreligious groups, regression analysis demonstrated the existence of interaction effects. However, they were not the specific interaction effects anticipated on the basis of status-inconsistency theory. Rather, we attribute our results to the persistence of subcultural differences within ethnoreligious groups in Detroit.

THE STATUS-INCONSISTENCY MODEL

That in complex societies an individual's position in one social ranking or status system does not necessarily determine or coincide with his location in other status systems has long been recognized in sociological theory (see Weber 1953; Sorokin 1947). While there is a tendency for "different types of status to reach a common level" over time (Benôit-Smullyan 1944, p. 160; cf. Kimberly 1970), at any given point in time there are always individuals whose several statuses are highly inconsistent. Since Lenski reintroduced the notion of discontinuities among status systems in his theory of status crystallization or status inconsistency (Lenski 1954), a wide range of studies have been undertaken to examine the possible correlates and consequences of such discontinuities (see Geschwender 1967).

The basic model implicit in the literature is a straightforward one. Through processes of social mobility and social change, people with low evaluated ascriptive status characteristics, such as race, religion, and

[1] We are indebted to our colleagues, Gudmund Iversen and Paul Siegel, for their advice on the statistical analysis and to our research assistants, Stephen J. Cutler and Gerald K. Hikel The total study and this analysis were supported by grants from the Horace H Rackham School of Graduate Studies, University of Michigan, and the National Science Foundation (GS-1929).

ethnicity, succeed in raising their positions in status systems based upon achievement, such as those of educational attainment or occupational prestige (Segal and Knoke 1968). This necessarily creates inconsistencies between their relative ranks in the achieved and ascribed status systems.

A person in such a situation is assumed to define his social position in terms of his higher status and will expect deference and other status-linked behavior from others to conform to the privileges befitting such higher status (Galtung 1966). People interacting with the inconsistent individual, perhaps in the interest of maximizing their own relative power in the relationship, however, may define him in terms of his lower status, and hence his deference expectations will be frustrated (Lenski 1966, p. 87). Empirical research suggests that, in fact, people will judge the status-inconsistent individual, not in terms of his lower status, but in terms of the average of his several statuses (Himmelfarb and Senn 1969). The net result, however, will be the same. Others will attribute to him lower status than he attributes to himself.

Lenski suggests that individuals in such a situation will be subjected to disturbing experiences in social encounters and will experience difficulty in establishing rewarding patterns of social interaction (Lenski 1956). The literature suggests that there are a variety of behavioral and psychological responses to such stress. Jackson (1962) sees it causing psychological disturbance. Goffman (1957) sees it leading to desires to change the distribution of power in society, presumably to restructure society in a way that will make the lower status of the status-inconsistent individual less relevant. Lenski (1954, 1967) demonstrates a relationship between status inconsistency and Democratic party support, as well as with liberalism with regard to socioeconomic issues.

CRITICISMS OF THE MODEL

Four major types of criticism have been directed at the status-inconsistency model. First, the assumed underlying dynamic, namely, disruption of social relations as a function of status inconsistency, has never been empirically demonstrated. Lenski (1956) presents only inferential evidence in support of this proposition, and Bauman (1968) presents data which suggest that status-inconsistent people may actually have more satisfying social contacts than do people who are status consistent.

Second, some critics have argued that when the main effects of the individual statuses are taken into account first, the statistical "interaction" effect due to status inconsistency has no explanatory power (Brandmeyer 1965; Treiman 1966) or only minimal power (Fauman 1968) with regard to the dependent variable.

Third, the effects attributed to status inconsistency have been inter-

preted by some researchers as being due to ethnic group membership. One variant of this criticism is concerned with the main effects of ethnic status as a variable in the stratification system, and in this wise, this argument is merely a specification of the above-mentioned point (Kelley and Chambliss 1966). Indeed, Lenski (1954) has argued that the most important inconsistencies for explaining liberalism are those that occur between low ethnic status and high financial, educational, or occupational status; and Segal (1969) has shown that the political effects of status inconsistency are in general manifest only when the low ascriptive status of the inconsistent individual is socially visible (cf. Box and Ford 1969). Another variant of explanations in terms of ethnicity takes a more subcultural bent. From this perspective, the relative social ranking of the ethnic groups is not important. Rather, primacy is placed upon their subcultural traits affecting political attitudes and sociability patterns (see Glazer and Moynihan 1963). It should be noted that while one of the authors anticipated finding evidence of status-inconsistency effects in our data, the other author felt that our results could be adduced to demonstrate the persistence of subcultural factors.

Finally, on methodological grounds, it has been argued that although some statistical interaction effects have been demonstrated between status variables with regard to some presumed effects of status inconsistency, any "interaction" effect cannot simply be equated with an inconsistency effect (Blalock 1967*b*; Hyman 1966; Mitchell 1964). Blalock (1966*a*, 1966*b*, 1967*a*) suggests in this regard that a priori specification of main effects as well as the expected magnitude and direction of interaction effects is necessary in identifying which interaction effects are attributable to inconsistency.

RESEARCH GOALS

The present study attempts to confront four unresolved questions raised in the above discussion:

1. Can the presumed effects of status inconsistency upon social relations and political attitudes be replicated on an all-white, native-born sample, or are these factors so dependent upon racial and nativity differences that they will be absent in a situation where race and nativity do not vary?

2. Do individuals who are objectively status inconsistent actually experience less intense social relationships and less frequent social contacts than do people who are status consistent? That is, do the specific forms of statistical interaction among objective status variables suggested by the theory of status inconsistency tell us something about social relations above and beyond what we know can be attributed to the main effects of

the status variables, giving us a basis for inference regarding the subjective processes involved?

3. Are there interaction effects on social participation and political attitudes apart from those attributable to status inconsistency?

4. If there are such effects, can they be attributed to the persistence of ethnoreligious or subcultural differences?

THE SAMPLE

Our data were collected during the spring and summer of 1966 in the greater metropolitan area of Detroit by the Detroit Area Study, University of Michigan. The sample consisted of 1,013 native-born white males between the ages of twenty-one and sixty-four, representing 80 percent of the eligible households sampled. Fourteen percent of the households originally sampled refused interviews, and an additional 6 percent was lost because no one was found at home after repeated callbacks or for other reasons. Since this sample is exclusively white, we were assured that if inconsistency effects were discovered, they could not be due to racial differences. At the same time, the fact that the sample was native born assured us that if we did discover ethnoreligious differences within the white sample, they would be attributable to persisting subcultural differences and not to differences between immigrants and native-born Americans.[2]

STATUS DIMENSIONS

Previous research has shown that the most important status inconsistencies are those between an achieved status and an ascribed status (Segal and Knoke 1968). Education was utilized as the achieved-status dimension in this analysis. Education is in general highly correlated with other achieved statuses; in our sample the correlation between education and occupation was .61. At the same time, the main effects of education on attitudes and social participation have repeatedly been demonstrated to be, in general, greater than the main effects of income or occupation for the population as a whole (e.g., Hodge 1970).

Race, religion, and ethnicity are the most commonly analyzed ascriptive bases of social status.[3] Race was precluded from the present analysis

[2] As a test of the assumption of persisting differences, we analyzed our data controlling for generational differences While we observed generational differences, they were not consistent in direction, and did not suggest the atrophy of subcultural differences

[3] Age is an additional ascriptive status that comes under frequent scrutiny. Smith (1969) has in fact argued that age is an important intervening variable in understanding the effects of status inconsistency In controlling our analyses for age, we found main effects attributable to age These effects, however, formed no coherent pattern across ethnoreligious groups

by the nature of the sample, and the utility of religion in and of itself has recently been called into question. Specifically, Gockel (1969) and Goldstein (1969) have argued that status differences among members of various American religious groups are predominantly functions of educational, occupational, and regional differences in their compositions, rather than of religious differences per se. However, at its inception, America was a Protestant nation, and remains so in the ideology of the Protestant plurality (Baltzell 1964). Moreover, the persistence of socioeconomic differences among religious groups, whatever they are attributable to, are reflected in their differing social standings.

Laumann (1969) has demonstrated that for the present sample, the structure of friendship choices can be described in terms of religious preference and socioeconomic status. More important, however, is his finding of ethnic differentiation *within* religious groups. This is consistent with research on the persistence of ethnic differentiation in America (see, e.g., Kantrowitz 1969) and with Miller's (1968) findings with regard to political variables which suggest that ethnic and religious factors must be looked at concurrently.

On the basis of these considerations, we defined our ascriptive status variable in terms of the positions of the fifteen largest ethnoreligious groups on the first axis defined in Laumann's (1969) smallest space analysis of these data. The rank-order correlation of this measure with Hodge and Siegel's (in press) ethnic status index was .668 ($P < .01$).[4] The correlation with mean occupational status of group was .248 (N.S.), indicating that our measure of ethnoreligious status was not simply an artifact of differences in socioeconomic status (see also Lasswell 1965, pp. 340-48). Twelve other groups defined by him were omitted from this study due to insufficient numbers to sustain analysis.

We viewed the propositions of Segal (1969) and Box and Ford (1969) regarding the visibility of ascribed status as a potential refinement of the status-inconsistency model. If visibility were not a requisite for the processes assumed by the model, we would expect to observe inconsistency effects for all low-status ethnoreligious groups as education increased. If visibility were a necessary condition, on the other hand, then on the basis of Segal's findings, we would expect only the Jews, as the most visible low-status group, to consistently manifest the hypothesized effects of status inconsistency. We must point out that both the theory of status

[4] While the correlation between Laumann's ethnoreligious status ranking and Hodge and Siegel's ethnic status index was not perfect, we are comforted by its magnitude The former involves objective interaction patterns, while the latter reflects subjective choices It is comforting to know that the two are related The departure from unity may be attributed to the fact that subjectively, each group ranks iself first, after which there is consensus on the relative ranking of other groups (Laumann, forthcoming).

inconsistency and the data with which we confront it are concerned with ascribed status in a heterogeneous community.

Status inconsistency may also have somewhat different effects on one's relations with his own ethnoreligious group than it does on his relations with members of other groups. The highly educated Jew, for example, may not be afforded deference on the basis of his education by non-Jews, but he will have high status within the Jewish community because of his adherence to the value placed on scholarship by that group.[5] The highly educated black, on the other hand, may both be refused deference by the white community and be alienated from the black community because of the difference between the modal level of education in that community and his own level. The intersection of the effects of subcultural patterns and the effects of the structure of the larger community needs to be explored more deeply than we are able to do with the data available to us.

THE ANALYTIC MODEL

Alternative hypotheses derived from the theory of status inconsistency, on the one hand, and the assumption of persisting subcultural differences, on the other, were operationalized in terms of the equation $Y_{ki} = a_k + b_k x_{ki} + e_{ki}$, where Y_{ki} is a measure on individual i in group k which may manifest the hypothesized effect of status inconsistency, for example, disrupted social relations or political liberalism; a_k is the intercept of ethnoreligious group k on the ordinate; b_k is the regression slope for ethnoreligious group k; x_{ki} is the educational attainment of the ith individual in group k; and e_{ki} is the error term.

At the first level of analysis, we were concerned with whether differences in political attitudes and social relations were attributable to educational differences, to ethnoreligious differences, or to both. If both variables have an effect on the dependent variable, we would then be concerned with whether those effects were simply additive or whether statistical interaction occurred. Finally, if interaction effects were detected, we were concerned with whether these effects were of the specific type predicted by status-inconsistency theory or whether they would be attributable to nonstatus characteristics of the groups included in the analysis (see Hodge and Siegel 1970). For example, Poles, as a group, have been shown to have disproportionate tendencies to own real estate (see Wood 1955; Wilson and Banfield 1964). As achieved status increases

[5] Our data suggest that neither retreat into nor withdrawal from the ethnoreligious community is a common response to status inconsistency. That is, status inconsistency is not related to the ethnoreligious homogeneity of social contacts. Although the level of ethnoreligious homogeneity varied among the groups we analyzed, as indicated by significant differences among the a intercepts (table 2), we found no significant differences among the regression slopes

among the Poles, they will presumably purchase more property and may become increasingly anxious about the security of their property. We might, therefore, find hostile attitudes toward groups perceived to be property threats, (e.g., Negroes) increasing as a function of achieved status, but attributable to subcultural rather than inconsistency factors (see Greeley 1968, who showed that Polish Catholics have the highest anti-Negro sentiments of Catholic groups tested).

Our statistical analysis had three objectives. First, we evaluated the differences among the slopes of the regression lines for the fifteen ethnoreligious groups by calculating the ratio of variance between slopes to variance within groups as a means of detecting the presence of interaction effects. Essentially, this test (Hald 1952, p. 580) determines whether the set of slopes of the ethnoreligious groups, b_k, varies appreciably around the common slope, $\bar{b}$. Second, we determined whether the fifteen regression lines were identical (i.e., coincident) or different from one another (Hald 1952, pp. 579–84). That is, the first test merely establishes whether the slopes, b_k, are equal.

It could be that the within-group relations of education to the dependent variable for each of the fifteen groups were equivalent, but that the regression lines themselves were not identical but parallel to one another (i.e., significant differences among the a-intercepts). Parallel lines would indicate the presence of group differences, *net of the educational differences* among the groups. Finally, the regression slope for each ethnoreligious group was compared to the weighted average of all fifteen slopes, using a method developed by Tukey (Acton 1966, pp. 184–87), to test for the significant departure of any specific group slope from the common slope. Several groups might deviate significantly from the common slope while all the others did not. Such a situation, especially if the deviating groups were numerically small, would not necessarily result in a significant F-ratio on the first test, but would be identified by the Tukey test.

The expectation was that if there actually were interaction effects due to status inconsistency, then, as a general pattern, for high ethnoreligious status groups (e.g., German Presbyterians and Anglo-American Methodists) the value of the dependent variable would decrease as education increased and the two statuses became increasingly consistent. The pattern for lower ethnoreligious status groups, on the other hand, would be deflected from this pattern.

This expectation is presented graphically in figure 1a, where the interaction effect is indicated by the difference beteween b_1 and b_2. As a function of the groups involved, this would indicate either inconsistency effects or interactive subcultural effects. If b_1 and b_2 were not significantly different but a_1 and a_2 were different, that is, the lines were parallel, this would indicate the presence of additive ethnoreligious group effects. This outcome

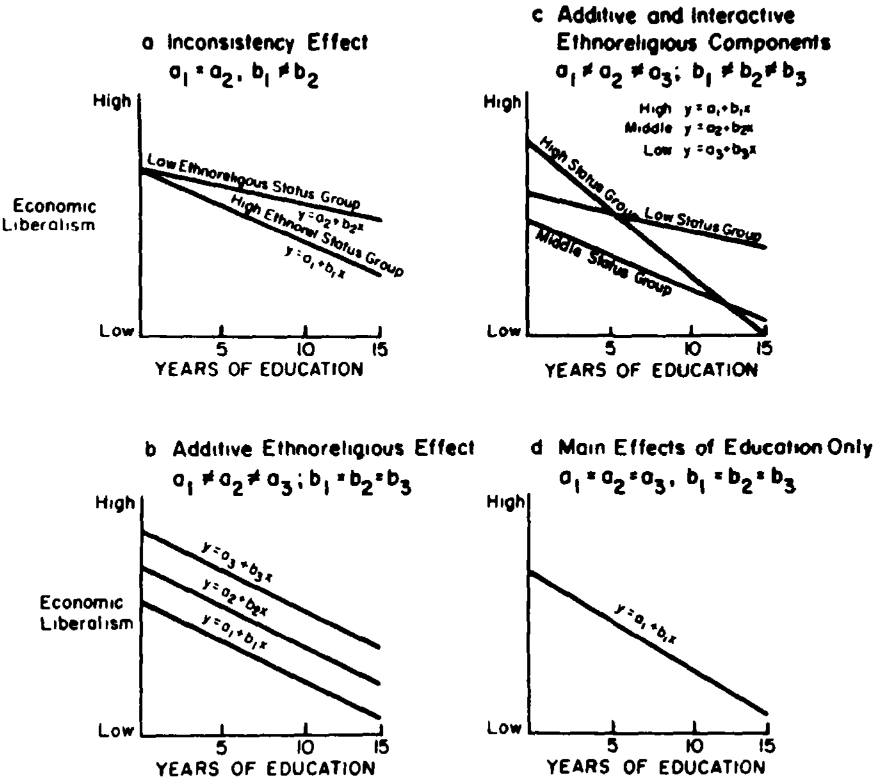


FIG. 1—Hypothetical regression of economic liberalism on education, under varying conditions of ethnoreligious effects

is represented graphically in figure 1*b*. We shall note here that these additive effects need not be related to the social status of the groups.

Figure 1*a* reflects an ideal typical set of findings which, while useful for purposes of exposition, would be unlikely to be approximated by a body of data. A more likely outcome, if status-inconsistency effects were operating, would be that the slopes of the several regression lines would be increasingly deflected from the pattern observed among high-status groups as social distance from those groups increased, but that the pattern would be confounded by main effects attributable to ethnoreligious group differences, reflected in differences in the *a*-intercepts. This kind of outcome is presented in figure 1*c*. Here again, the additive ethnoreligious component is indicated by the significance of the differences among a_1, a_2, and a_3, and the presence of interaction due to status inconsistency is indicated by the significance of the differences among b_1, b_2, and b_3. This sort of pattern, with any other status ordering of the groups, would indicate interaction effects due to factors other than status inconsistency.

Finally, our statistical analysis might have yielded no ethnoreligious component at all, whether additive or interactive. Such a situation, where the only effects are the main effects of education, is presented in figure 1*d*.

FINDINGS

The fifteen groups that were subjected to analysis are presented according to their rank ordering on the ethnoreligious dimension in table 1. Mean

TABLE 1

ETHNORELIGIOUS, OCCUPATIONAL, AND EDUCATIONAL STATUS OF FIFTEEN ETHNORELIGIOUS GROUPS

Rank on Ethnoreligious Status Scale*	Group	Total	School Years		Occupational Status†	
			$\bar{X}$	SD	$\bar{X}$	SD
Protestant Groups						
1	German Methodists	32	13 0	3 24	50 4	25 37
2	German Presbyterians	25	13 8	2.94	58 0	24.92
3 ...	Anglo-American Methodists	40	11 4	3 21	46 5	25 68
4	Anglo-American Presbyterians	72	13 7	2 83	59 2	21 65
5	German Lutherans	57	12 2	3 30	49 9	24 90
6	Anglo-American Baptists	80	10 2	3 08	36 0	22 93
7	Protestants, Origin N A	32	9 5	3 81	32 0	18 86
Catholic Groups						
8	Italian Catholics	55	12 0	3 00	44 1	23 24
9	Anglo-American Catholics	33	11 2	3 83	43 3	27 65
10	Irish Catholics	65	12 7	2 93	51 1	21 63
11	German Catholics	81	12 2	2 75	48 6	23 62
12	French Catholics	51	12 0	3 19	41 2	22 92
13 ..	Slavic Catholics	38	12 3	3 25	45 5	25 27
14	Polish Catholics	111	11 0	3 55	39 6	22 87
15	Jews	29	14 8	2 84	63 4	24 22
Total		801	12 0	3 18	46 3	23 42

NOTE --N A = origin not ascertained on all tables

* Rank on the first axis of the three-dimensional smallest space analysis of ethnoreligious groups (Laumann 1969, p 194)

† The current occupation of the respondent was first coded into the six-digit detailed 1960 occupation-industry code of the U S Bureau of the Census and then recoded by computer to the two-digit code of Duncan's Index of Socioeconomic Status (see Duncan 1961)

number of school years completed and mean occupational status, as well as their standard deviations, are also presented for each group. The rank-order correlation between mean occupational status and mean number of school years completed was .94.

The basic findings relevant to our discussion of the theory of status crystallization versus the theory of ethnic or subcultural differences are summarized in table 2. We have divided the dependent variables into two sets.[6] The first set includes five measures intended to tap various aspects

[6] In the Appendix we have provided the means and standard deviations for all the

of a man's orientation to political and economic issues—it being assumed in the literature (cf. Fauman 1968; Goffman 1957) that "liberal" orientations (specifically, preference for the "underdog," the worker, in labor-management disputes and active governmental support in guaranteeing job opportunities, Democratic party preference, and civil libertarianism) are especially likely to be expressed by status-inconsistent individuals. We further reasoned that a man's condition of status discrepancy on two important ranking dimensions in our society might make him especially concerned with social status matters per se, that is, concerned with how other people regarded him. As the first column in the table shows, high educational attainment is associated with conservative economic ideology, Republican party preference, willingness to grant civil liberties to Ku Klux Klansmen and Communists, and low status concern.

The second set of dependent variables is intended to tap various aspects of a man's social participation in intimate primary groups and secondary or voluntary associations. The theory of status crystallization leads one to expect that status-discrepant individuals are likely to be subject to considerable strain in engaging in social relations with others because their social ambiguity, by which they may be accorded high or low deferential treatment depending on which status dimension others choose to regard as more important in the interactional context, creates anxiety concerning their preferred status treatment (naturally they would always like to be treated in terms of their more highly evaluated status). Consequently, a status-discrepant man is expected to avoid membership in voluntary associations which are broadly recruited (that is, include members who are not also members of the disvalued status), to be less involved in those voluntary associations in which he is a member, and to prefer friendship groups which are strictly comparable with him in status attributes (thus, such an individual would prefer kin-based and ethnoreligiously homogeneous friendship networks). We might also expect that the reported closeness of friendships, the frequency of interaction with friends, and the duration of friendships would be adversely affected for such individuals as manifestations of their "defensive," ego-protecting posture toward social relations.

The alternative theory of ethnic differences would simply maintain that ethnoreligious groups will differ on both sets of measures, once socioeconomic differences in their composition are taken into account, because each has a unique cultural and historical relationship to American society that mediates their members' relation to it. Thus, given the different times

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dependent variables for the fifteen groups. The variables are defined in the footnotes to table 2 Additional documentation is available from the authors and will be published in a monograph (Laumann, forthcoming).

TABLE 2

TESTS FOR DIFFERENCES AMONG VARIOUS REGRESSION PARAMETERS FOR FIFTEEN ETHNORELIGIOUS GROUPS FOR SELECTED POLITICAL AND SOCIAL ATTITUDES AND MEASURES OF SOCIAL PARTICIPATION, REGRESSED ON EDUCATIONAL ATTAINMENT

Dependent Variables	Product-Moment Correlation (r) (1)	Test for Differences among Slopes, b's (2)	Test for Differences among Intercepts, a's (3)	Groups Deviating Significantly from Common Slope, $\bar{b}$, according to Tukey Test (4): Steeper than Common Slope	Groups Deviating Significantly from Common Slope, $\bar{b}$, according to Tukey Test (4): Flatter than Common Slope
A Political and social attitudes					
Economic ideology[a]	26*	N S	$F = 2\ 58, P < .05$	Anglo-Amer Meth., Prot., Origin N A.	German Pres , Jews
Party preference[b]	− 23*	N S	$F = 10\ 43, P < 001$	Anglo-Amer Cath.	German Pres., Jews
Tolerance for Ku Klux Klan[c]	−.18*	N S	N S	None	None
Tolerance for Communists[c]	− 30*	N S	N S.	None	None
Status concern[d]	−.20*	N S	N.S	German Pres , Jews, Prot , Origin N A	Irish Cath , Slavic Cath
B. Social participation.					
No of memberships in voluntary associations[e]	39*	N S	N S	None	None
Degree of associational involvement[f]	03	N S	$F = 2\ 94, P < 05$	None	None
Average closeness of friendships[g]	− 11*	N S	N S	None	None
Average frequency of interaction[h]	− 01	N S	N S	None	None
Average duration of friendships[i]	− 02	N S	$F = 2\ 05, P < .10$	Anglo-Amer Bapt , Polish Cath., Jews Prot., Origin N A.	German Pres , German Meth., Anglo-Amer. Cath.
No. of work-based friends[j]	− 02	N S	N S.	None	None
No. of kin-based friends[k]	− 13*	N S	N S.	Slavic Cath , Jews	Anglo-Amer. Cath
Mean ethnoreligious homegeneity[l]	−.05	N S	$F = 32.147, P < .001$	None	None
Mean occupational homogeneity[l]	−.00	N S	N.S	Jews	None

[a] Based on answers to three questions (1) ' Which of these four statements do you come closest to agreeing with? (a) Labor unions in this country are doing a fine job (b) While they do make some mistakes, on the whole labor unions are doing more good than harm (c) Although we need labor unionism in this country, the way they are run now they do more harm than good (d) This country would be better off without any labor unions at all " (2) "Which statement comes closer to your own opinion? (a) The most important job for the government is to make certain every person has a decent steady job and standard of living (b) The most important job for the government is to make certain that there are good opportunities for each person to get ahead on his own " (3) "Going back to some general opinion questions, in strikes and disputes between working people and employers, do you usually side with workers or with the employers " (Cf Laumann 1966, pp 182–84, for details on index construction) The range on this index was from 1 (liberal) to 7 (conservative)

[b] Party preference was coded on a seven-point scale from "strong Republican" (1) through "independent" (4) to "strong Democrat ' (7)

[c] Based on answers to five questions, each coded as a tolerant (1), neutral (2), or intolerant (3) response and average (1) "Suppose there is a man who admits he is a Communist (Ku Klux Klansman) Suppose this admitted Communist (Klansman) wants to make a speech in your community Should he be allowed to speak or not?" (2) "Should an admitted Communist (Klansman) be put in jail?" (3) "Suppose he is a teacher in a high school Should he be fired or not?" (4) "Suppose he is a clerk in a store Should he be fired or not?" (5) "Now I would like you to think of another person A man who has been questioned by a Congressional Committee about his suspected Communist (KKK) sympathies, but who swears under oath he has never been a Communist (KKK) Suppose he is a teacher in a high school Should he be fired or not?" (cf Stouffer 1955, Laumann and Schuman 1967)

[d] Based on the summation of two Likert-scale items, each with five response categories (1) "It is worth considerable effort to assure one's self of a good name with the right kind of people " (2) "The raising of one's social position is one of the more important goals of life " The range of this index was from 0 (low status concern) to 8 (high status concern)

[e] The respondent was presented with the following list of voluntary associations and was asked to indicate to which ones he belonged church-connected groups (but not church itself) (19 1 percent), labor unions (39 3 percent), veterans' organizations (10 7 percent), fraternal organizations or lodges (22 7 percent), business or civic groups (9 6 percent), parent-teacher associations (15 8 percent), community centers (1 7 percent), organizations of people of the same nationality (2 4 percent), sports teams (16 6 percent), country clubs (5 9 percent), youth groups (scout leaders, etc) (8 3 percent), professional groups (13 9 percent), political clubs or organizations (5 5 percent), neighborhood improvement associations (10 9 percent), charity or welfare organizations (4 6 percent), and others (specified) (20 4 percent) (Percentages in parentheses are proportions of total sample who belong to given type of organization) It should be noted that very few of the organizations to which our respondents belonged could be presumed to be ethnoreligiously homogeneous Most secondary association memberships would imply some contact with members of other ethnoreligious groups, although the range of such groups might be limited

[f] For each organization in which the respondent indicated membership, he was asked whether he was very involved or not very involved in the activities of the organization

[g] For each of the three friends mentioned by the respondent, he was asked whether he was (1) a very close, personal friend (2) good friend, or (3) acquaintance

[h] For each of the three friends, the respondent was asked "All in all how often do you usually get together with (friend)? (1) more than once a week, (2) once a week, (3) two or three times a month, (4) once a month, (5) several times a year, (6) rarely "

[i] For each of the three friends, the respondent was asked how many years he had known the friend We determined the proportion of a man's life he had known the friend by dividing the number of years he had known the friend by the age of the respondent

[j] For each of the three friends, we asked the respondent "Do you see (friend) regularly where you work—that is, at least once or twice a week?"

[k] For each friend, we asked the respondent whether the friend was a relative (consanguineal or affinal) of his

[l] For each of the friends, we asked the respondent to report his occupation and ethnic origin and specific religious preference On the basis of the smallest space analyses of the structures of friendship choices among occupations (Laumann, forthcoming) and among ethnoreligious groups (Laumann 1969), we determined the distance of each friend's group from the respondent's and averaged these three distances for a measure of the homogeneity of the friendship network (cf Laumann, forthcoming)

* $P < 01$

of arrival of the various ethnoreligious groups and cultural and other peculiarities of their European and American experiences (cf. Handlin 1959; Higham 1955; Lieberson 1963), we should expect differences among the groups with regard to many social attitudes and modes of social participation. A number of studies (e.g., Glazer and Moynihan 1963; Wilson and Banfield 1964; Dahl 1961) have suggested, for example, that certain groups, such as the Irish Catholics, have especially strong ties to the urban-based Democratic party as a result of their concentrations in urban ghettos of our eastern cities in the mid-nineteenth century.

The last four columns of table 2 present the summary results of our regression analysis. As noted above, we calculated the regression equation, $Y_{ki} = a_k + b_k x_{ki}$, where x_{ki} is the number of school years completed by each man, for each of the fifteen ethnoreligious groups. We first determined whether the slopes, b's, were significantly different from the common slope, $\bar{b}$, for the men irrespective of ascriptive group membership. As is readily apparent from a glance at column 2, all of the F-tests failed to indicate significant differences among the slopes. Considering this evidence alone, we would be forced to conclude that the relationship of educational attainment to the various dependent variables, as measured by the slopes of the regression lines, do *not* differ among the fifteen groups—that is, there are no detectable patterns of interaction effects of education and ethnoreligious group on the dependent variables. As noted before, the theory of status crystallization at the least implies the presence of statistical interaction effects and, more specifically, interaction effects which are patterned such that low evaluated status groups are expected to be deflected from the "normal" relationship of, for instance, conservatism with high status, toward more liberal positions. Unfortunately for the theory, however, we do not find any significant interaction effects according to this first test (in col. 2).

Our theory of ethnic group differences, however, fares much better. While it may be true that all the groups manifest similar relationships of educational attainment to the dependent variable, it is still quite possible that the groups differ significantly among themselves with regard to their tendency to be high or low on the dependent variable, even when the effects of their differences in educational composition are taken into account. For example, Irish Catholics, on the average, tend to be more inclined to the Democratic party than German Methodists, although their within-group relationship of education to party preference is the same. In the regression analysis this would be reflected by the two groups having parallel rather than coincidental regression lines, that is, the a's or Y-intercepts would be significantly different. As column 3 indicates, five of the F-tests for determining the presence of noncoincident regression

lines are statistically significant. With respect to political and social attitudes, Jews and Catholic groups of the recent or "new migration" generally tend to be more heavily Democratic in party preference and economically liberal than "old migration" Protestant and Catholic (e.g., French and Anglo-American) groups. Due to limitations of space, we have included only one graphic portrayal (see fig. 2) of the regression lines—that of political party preference and educational attainment for the fifteen ethnoreligious groups; but the patterns observed here are generally

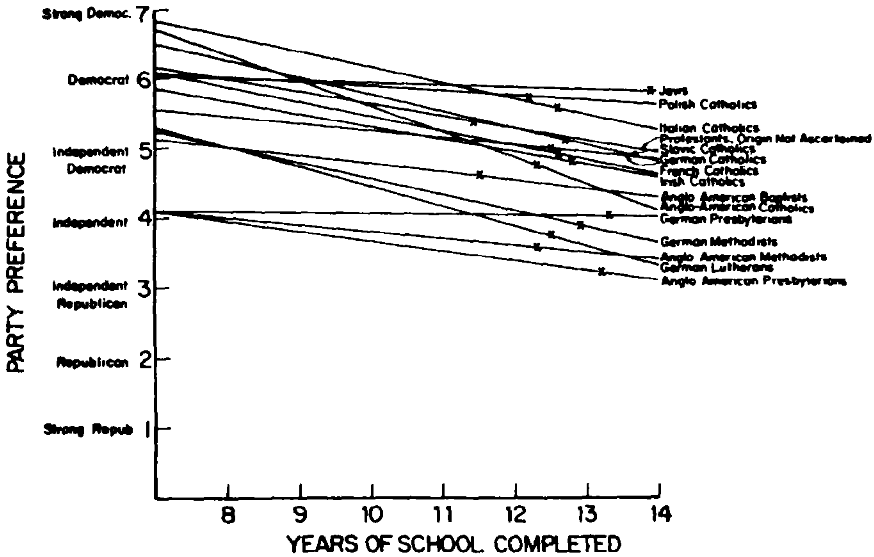


FIG. 2—Regression lines of political party preference by educational attainment, for the fifteen ethnoreligious groups

replicated for the other four measures of political and economic views. Note that the status ordering of the ethnoreligious groups in terms of partisanship at the high end of the education dimension is almost exactly what we would expect in terms of status-inconsistency theory. It is the processes producing this ordering that make explanations in terms of status-inconsistency theory questionable Rather than finding ethnoreligious status to be related to the deflection of regression lines, we find that in some instances the lines for high and low groups are almost parallel (e.g., German Presbyterians and Jews), while some low-status groups manifest deflections *toward* the lines observed for higher-status samples (e.g., Italian Catholics).[7] The observed status ordering, then, must be

[7] Rank-order correlations were computed between the ethnoreligious status ranks for the fifteen groups (see table 1) and their regression slopes for each of the fourteen dependent variables. These correlations can be taken as summary measures of the

explained in terms of the initial ordering of groups, that is, on the intercepts at the ordinate, as much as in terms of regression slopes.

With respect to our measures of social participation, Jews and new-migration Catholics did not differ greatly in their involvement in voluntary associations from old-migration Protestants and Catholics. They were, however, more likely to have kin-based and ethnoreligiously homogeneous networks as alternative areas of sociability, thus making the voluntary associations relatively more important to the old-migration groups, which lacked these ascriptive networks (see the appendix).

Column 4 reports the results of comparing specific group slopes with the common slope to detect significant differences, utilizing Tukey's (in Acton 1966, pp. 184–87) technique for calculating confidence intervals for the discrepancy between the group slope and the common slope. Column 2 reported the results of a test that determines whether the set of slopes varies appreciably around the common slope. In the latter case, if there are a number of groups that do not differ among themselves, the test is likely to fail to be very much affected by the presence of several groups that do in fact differ from the common slope but are not sufficiently numerous to affect the overall pattern. The Tukey technique was developed to permit the detection of specific group departures from the common slope. To facilitate discussion and interpretation, the groups in column 4 have been divided into those which are significantly steeper than the common slope and those which are significantly flatter than the common slope.

There were eight dependent variables for which we found significant deviations of group slopes from the common slope. Most of the groups so identified have few members; consequently, caution should be exercised in drawing conclusions. There is one readily observable pattern, however. German Presbyterians and Jews both appear to deviate rather regularly from the common slope and in the same way, in contrast to a somewhat more varied set of Protestant and Catholic groups. These latter have middling achieved status as groups (see table 1) and usually deviate in the opposite direction from the German Presbyterians and Jews.

It is especially noteworthy that German Presbyterians and Jews both enjoy the highest mean occupational and educational status and family income of any group in the sample—that is, on achievement-based criteria, they are the two most favored groups in the Detroit metropolitan

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presence of the patterns we would expect if status-inconsistency effects were operating. If these effects were present, we would expect the rank-order correlations to be positive in sign, that is, the highest ranked groups would have the steepest slopes and the lowest ranked groups the flattest slopes. Contrary to this pattern, we found that only two of the fourteen correlations—number of voluntary association memberships and average duration of friendships—were significantly different from zero at the .05 level. One significant difference from zero would have been anticipated on the basis of chance alone

area. Moreover, they enjoyed these highly favored achieved statuses over two generations, with their fathers also the highest in mean educational and occupational status (cf. Laumann, forthcoming). While most other Protestant and Catholic groups manifested considerable upward intergenerational mobility, these groups were already at or near the top of the achieved status system a generation ago.

But what is of signal importance in this context is the fact that German Presbyterians enjoy exceptionally high ascriptive status while Jews are at the bottom of the white ethnoreligious hierarchy of social status (see above discussion on ethnoreligious group ranking). Yet the relationship between achieved socioeconomic status, as measured by education, and various attitudes and behaviors are similar for these two groups and differentiate them from the rest of the population. In the cases of economic ideology and party preference, higher socioeconomic German Presbyterians and Jews are less likely to be economically conservative and Republican than high socioeconomic status members of other groups.[8] While the theory of status crystallization would predict such patterns for Jewish high achievers, such patterns for German Presbyterians are completely contrary to the theory's predictions.

Certainly a plausible inference from the theory would be that high status-discrepant individuals would have higher status concern than status-consistent individuals. But another "blow" to the theory might be seen in the fact that among both Jews and German Presbyterians the negative relationship between education and status concern is high, relative to other groups. That is, the regression slopes for these two groups are steeper than the common negative slope for the sample as a whole.

Most of the groups identified as having slopes significantly higher than Jews and German Presbyterians with respect to economic ideology, party preference, and status concern are of middle rather than high or low achieved status and have had unusually high rates of upward intergenerational mobility (Laumann, forthcoming). It is higher socioeconomic status members of these groups who are more likely to be economic conservatives and Republicans and to express high status concern. In a purely speculative vein, we suggest that perhaps the status insecurity engendered by such intergenerational mobility may be seen to promote more conservative political and economic attitudes in an effort to protect recently acquired gains (cf. Lopreato 1967). For groups like Jews and German Presbyterians who tend to have greater intergenerational status stability, such defensive measures are not seen as particularly relevant.

[8] As can be seen in figure 2, however, it should be noted that Jews as a group are more likely to have Democratic party preferences than German Presbyterians as a group, who tend to be independent Republicans. Both groups, however, have roughly equal means on the economic ideology scale.

To summarize the results to this point, we have shown that while there are substantial differences among ethnoreligious groups on a number of political and social attitudes and characteristic modes of social participation, net of group differences in educational composition, we have also suggested that the theory of status crystallization affords little if any explanatory power in accounting for the pattern of differences among groups. The sole exceptions to this generalization are status-inconsistent Jews, who behave in large part the way the theory of status inconsistency predicts that they should. Since our so-called theory of subcultural differences merely asserts the presence of group differences without specification of their form, we can only conclude that the observed patterns (see the Appendix) should provide useful clues for the construction of a model of ethnoreligious group differences which would have to be tested on other sample populations.

In the course of our review of the literature on status crystallization, we concluded that one possibly important intervening variable linking a man's objective condition of status discrepancy to his subjective view of the world and social behavior would be his degree of awareness of or concern for status that would presumably make him more or less sensitive to the status-linked behavior and attitudes of others. In an earlier work (Laumann 1966, pp. 105–22), the senior author observed: "The phenomenon of *differential status awareness* is an important attribute of stratification systems in its own right that may have significant consequences for processes occurring within the status system and its relation with other institutional subsystems, such as the political or economic system" (p. 106). While the measure of differential status awareness employed in this earlier study was derived from the respondents' differential tendencies to discriminate subjectively among men in various occupations as possible partners for intimate social relationships (such as fathers-in-law, friends, and neighbors), it is highly likely that this measure would be positively related to the measure of status concern employed in this paper. In general, it was found that:

> Status-sensitive men tend to be those who identify themselves as members of the upper and upper-middle classes or those who derive from ethnic groups that have more recently arrived on the American scene They are likely to desire their intimate associates to be of comparable status to themselves and are likely to succeed in confining their relations to such persons. Moreover, their theories of the bases of the class structure emphasize its hierarchical character. They are themselves upwardly mobile and aspire for upward mobility for their children . . they express stronger political party preferences for either the Democratic or Republican parties and have more extreme and well-defined economic ideologies of either a liberal or conservative persuasion. [Laumann 1966. p 121]

Indeed, using a measure of status crystallization comparable with Lenski's (1954), it was even found that status-discrepant men tended to be more status sensitive.

One might reasonably speculate that men with high status concern should manifest the effects of status discrepancy prescribed in the theory of status crystallization more clearly than those who were unconcerned with such matters. To test this speculation, we performed a similar analysis to that described above for a subsample of those men who scored roughly in the upper third on our measure of status concern (see fn. d, table 2 [$N = 364$]). Table 3 summarizes the results for the Tukey test for identifying groups deviating from the common slope. Despite the sharply reduced sample sizes of the various groups (which should decrease the number of significant results due to increased sampling variability), we found that there were significant group differences for every one of the dependent variables (in contrast note that seven of the dependent variables in table 2 showed no significant results). This suggests that the presence of high status concern is an important precondition for eliciting interaction effects of educational attainment and ethnoreligious group membership. In the case of the first set of attitudinal variables, there are no changes in the pattern of group departures from the common slope when compared to the total sample, although there were some groups added to the list. We now, moreover, observe significant group departures from the common slope for the two measures of tolerance toward extremist groups of the right and left.

But perhaps the most noteworthy and puzzling results occur for our second set of measures relating to social participation. For every one of our measures, we observe groups deviating from the common slope that suggests the presence of significant interaction effects of educational attainment and group membership for men high in status concern. Unfortunately, the pattern of departures defies any summary generalization beyond the negative conclusion that the pattern predicted by the theory of status crystallization does ***not*** manifest itself for any of the dependent variables. It is not at all clear, for example, why German Lutheran and French Catholic men high in status concern should manifest significantly stronger relations between education and the number of memberships in voluntary associations while Anglo-American Baptist and German Methodist men, among others, should manifest significantly weaker relationships between education and associational memberships than the relationship obtained for the sample as a whole. It is certainly plausible to argue that if there are persisting differences among men of differing ethnoreligious groups in the ways in which they handle interpersonal relationships, they would be especially likely to manifest themselves among those members who are highly concerned with status. And in fact we do

TABLE 3

GROUPS DEVIATING SIGNIFICANTLY FROM COMMON SLOPE, $\bar{b}$, ACCORDING TO TUKEY TEST FOR MEN WITH HIGH STATUS CONCERN

Dependent Variables	Product-Moment Correlation (r)	Groups Deviating Significantly from Common Slope, $\bar{b}$, according to Tukey Test: Steeper than Common Slope	Flatter than Common Slope
A. Political and social attitudes:			
Economic ideology	29**	German Lutherans	German Pres., Jews, French Cath.
Party preference	— 24**	German Lutherans, German Meth Anglo-Amer. Cath.	German Pres , Jews, German Cath , Prot , Origin N A
Tolerance for Ku Klux Klan	— 16**	German Meth , Anglo-Amer Cath	German Pres , Italian Cath , Slavic Cath , Prot , Origin N A.
Tolerance for Communists	— 21**	German Meth , Prot , Origin N A.	German Pres
B Social participation.			
No of memberships in voluntary associations .	30**	German Lutherans, French Cath	Anglo-Amer Bapt , German Meth , Anglo-Amer Cath , Jews, Prot , Origin N.A.
Degree of associational involvement	— 26**	None	Jews
Average closeness of friendships	— 04	German Meth , Irish Cath , Prot , Origin N A.	Anglo-Amer Meth., Anglo-Amer Cath.
Average frequency of interaction	03	German Pres , German Meth , Anglo-Amer Cath , Prot , Origin N A	Anglo-Amer Meth Slavic Cath.
Average duration of friendships	— 11*	Anglo-Amer Meth , Jews	Irish Cath
No. of work-based friends	— 08	None	German Meth , Anglo-Amer Cath , Slavic Cath
No. of kin-based friends ..	— 37**	None	None
Mean ethnoreligious homogeneity	— 04	Jews, Prot , Origin N A	Anglo-Amer. Bapt.
Mean occupational homogeneity	03	None	German Lutherans, Slavic Cath , Prot , Origin N.A

* $P < .05$.
** $P < .01$.

find that our measure of status concern is positively associated with subjective ethnic interest and traditional values concerning the family,[9] another measure presumed to be linked to the retention of strong ethnic orientations. But again, in the absence of a well-established model of ethnoreligious group differences that is based on a comparative study of these groups, we are left at the rather unsatisfactory point of asserting that the differences reported here should provide useful guidelines in developing such a model.

SUMMARY AND CONCLUSIONS

Our data clearly provide a basis for rejection of a general status inconsistency phenomenon such as that proposed by Lenski. There is no overall pattern of statistical interaction between ethnoreligious group membership and education with regard to either political attitudes or social participation. At the same time, there are significant interaction effects involving particular ethnoreligious groups that indicate subcultural differences in political orientations and sociability patterns. It is especially noteworthy that such effects are common among Jews, since previous research has suggested that due to their social visibility, Jews should be especially prone to the stresses of status inconsistency. At the same time, the similarity of interaction effects among German Presbyterians and Jews suggests that the stability of high achieved status in these groups across generations, and the relative homogeneity of these groups, may be more parsimonious explanations of their similarities that is the theory of status inconsistency, which in fact would lead us to expect differences in the very areas in which similarities have been observed. However, the patterns we have observed among the Jews in our sample, taken together with Segal's earlier results on nonwhites, suggests that similar behavior on the part of members of these two minority groups may well explain previous results that have been taken as supportive of the status-inconsistency argument. Moreover, as we have shown in figure 2, it is possible for apparent effects of inconsistencies between low-ascribed and high-achieved statuses to emerge on the basis of social dynamics other than those assumed by the theory of status inconsistency. Specifically, it is the persistence of traits characteristic of ethnic subcultures that leads to these results.

[9] Family traditionalism was measured by a three-item scale (1) "A wife should not expect her husband to help around the house after he's home from a hard day's work" (2) "Most of the important decisions in the life of the family should be made by the man of the house." (3) "In general, husbands and wives share in deciding matters that are important to the family's future" (see Goldberg and Litton 1968)

The incidence of statistical interaction effects increased in our data when we confined our analysis to men who scored high on our index of status concern. These interactions once again failed to fit the pattern predicted by the theory of status inconsistency. They do point to the necessity of rejecting the "melting-pot" theory of ethnic assimilation and recognizing the cultural pluralism of American society.

It is noteworthy that while our data do not support the melting-pot view of assimilation, neither do they support Borhek's (1970) alternative model of the breakdown of ethnic group cohesion. Borhek suggests that formal education is the most important determinant of both the homogeneity of friendship choices and the expression of assimilationist attitudes. While we have not dealt directly with the latter variable, we have regressed ethnoreligious homogeneity on education for each group and for the total sample. While we did find main effects due to ethnoreligious group membership, that is, significant differences among intercepts, we found the zero-order correlation between education and homogeneity to be statistically insignificant. Moreover, there were no significant differences among the regression slopes of the fifteen groups tested.

(See Appendix on next page)

Selected Political and Social Attitudes and Measures of Social Participation

Ethnoreligious Group	Economic Ideology		Party Preference		Tolerance for KKK		Tolerance for Communist		Status Concern		N Associations		Degree of Association Involvement	
	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD
Protestant														
German Methodists	4 81	1 71	3 88	2 18	1 41	0 45	1 59	0 43	4 21	1 86	2 2	1 92	174 1	51 7
German Presbyterians	4 46	1 38	4 00	2 48	1 49	0 46	1 54	0 54	3 96	2 21	2 7	1 77	175 9	50 6
Anglo-American Methodists	3 93	1 70	3 54	2 18	1 37	0 50	1 89	0 62	4 51	2 14	2 3	1 37	154 7	50 2
Anglo-American Presbyterians	4 81	1 65	3 19	2 00	1 42	0 44	1 62	0 55	3 85	2 34	2 4	2 14	178 9	50 4
German Lutherans	4 35	1 51	3 73	2 14	1 36	0 38	1 64	0 50	4 27	2 02	2 1	1 72	180 5	49 2
Anglo-American Baptists	3 97	1 45	4 58	2 12	1 51	0 55	1 95	0 60	4 88	2 06	1 6	1 39	176 5	52 7
Protestants, origin not ascertained	3 57	1 28	5 38	1 94	1 38	0 44	1 84	0 68	5 50	2 18	1 5	1 17	166 7	51 3
Catholic														
Italian Catholics	3 76	1 70	5 54	1 88	1 53	0 48	1 81	0 55	4 98	2 24	2 1	1 59	175 5	112 6
Anglo-American Catholics	4 06	1 27	4 73	2 05	1 53	0 49	1 75	0 57	4 59	1 78	2 2	1 87	177 7	51 1
Irish Catholics	4 19	1 41	4 77	2 00	1 48	0 43	1 80	0 63	4 28	2 17	2 5	1 82	176 9	52 0
German Catholics	4 27	1 44	4 95	1 94	1 46	0 49	1 72	0 56	4 71	2 15	1 8	1 69	182 8	98 6
French Catholics	3 71	1 54	4 92	2 13	1 49	0 37	1 88	0 57	4 53	2 33	1 9	1 57	163 8	52 1
Slavic Catholics	3 84	1 73	5 11	2 07	1 42	0 47	1 65	0 60	4 65	2 20	1 8	1 60	184 0	47 8
Polish Catholics	3 69	1 55	5 72	1 72	1 56	0 57	1 87	0 58	4 81	2 35	2 1	1 63	172 6	51 4
Jews	3 86	1 63	5 79	1 29	1 46	0 44	1 36	0 38	3 69	2 71	3 6	2 18	189 3	130 5
Grand total	4 08	1 54	4 70	1 99	1 47	0 48	1 76	0 57	4 54	2 20	2 1	1 70	175 5	67 4

APPENDIX (*Continued*)

Ethnoreligious Group	Average Closeness of Friends		Average Frequency of Interaction		Average Duration of Friendships		N Work-based Friendships		N Kin-based Friendships		Mean Homogeneity: Ethnoreligious		Mean Homogeneity: Occupational	
	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD
Protestant:														
German Methodists	1.4	0.47	3.0	1.15	12.9	6.40	0.45	0.78	0.38	0.61	99.7	23.92	49.7	30.5
German Presbyterians	1.4	0.49	2.8	1.22	13.6	8.10	0.46	0.93	0.28	0.54	67.1	22.03	38.8	26.0
Anglo-American Methodists	1.3	0.41	3.2	1.34	17.3	11.46	0.49	0.78	0.28	0.65	52.8	18.71	49.2	20.5
Anglo-American Presbyterians	1.4	0.44	3.0	1.15	14.2	8.06	0.39	0.72	0.31	0.60	43.5	17.48	46.2	25.6
German Lutherans	1.6	0.45	3.4	1.19	15.6	9.15	0.29	0.57	0.70	0.89	46.2	21.27	53.3	30.0
Anglo-American Baptists	1.5	0.49	2.7	1.32	11.8	6.69	0.56	0.90	0.48	0.70	66.9	32.12	45.4	27.2
Protestants, origin not ascertained	1.6	0.48	3.2	1.17	11.0	6.45	0.83	1.07	0.47	0.76	32.9	21.71	33.1	28.9
Catholic:														
Italian Catholics	1.3	0.43	2.6	1.22	13.8	8.77	0.50	0.87	0.53	0.86	34.0	18.44	53.1	27.8
Anglo-American Catholics	1.6	0.50	3.1	1.65	14.6	9.75	0.55	0.90	0.47	0.62	28.8	13.65	49.5	26.4
Irish Catholics	1.6	0.49	2.9	1.27	12.7	6.70	0.55	0.90	0.40	0.68	33.7	21.33	46.7	29.8
German Catholics	1.5	0.55	2.8	1.38	12.6	7.36	0.62	0.93	0.52	0.73	33.7	17.0	42.1	26.4
French Catholics	1.5	0.43	3.1	1.23	16.0	7.60	0.29	0.54	0.55	0.78	42.2	15.94	46.5	28.6
Slavic Catholics	1.6	0.48	3.2	1.36	15.4	9.28	0.33	0.76	0.70	0.85	50.4	21.92	55.9	26.0
Polish Catholics	1.6	0.46	3.0	1.22	14.6	8.22	0.32	0.71	0.60	0.85	31.9	23.95	46.5	26.6
Jews	1.5	0.48	2.7	1.21	15.8	11.54	0.33	0.73	0.46	0.86	24.2	44.45	40.3	37.4
Grand total	1.5	0.47	3.0	1.27	14.0	8.26	0.46	0.81	0.49	0.75	43.9	22.87	46.7	27.7

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The Future of Social Mobility Studies

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Despite decades of theorizing and researching, sociologists are starved for explanatory ideas. Consequently, a new or revived idea—like alienation, deferred gratification, social mobility, or status inconsistency—is often seized upon and overused, perhaps abused. Many concepts have a similar natural history. After initial high popularity in the professional literature, some wavering occurs because the findings of various studies do not mesh. Then, methodological criticism severely wounds the concept, for few ideas and studies can withstand the forceful sophistication of contemporary methodologists. Disillusionment with the concept is likely to follow. Occasionally, some hardy investigators partially rescue the concept and show how it might have some vitality. Some others continue to use it as though it had not suffered any injury.

The three studies reported in this issue seem to be in the disillusionment–partial rescue stages, difficult exercises indeed. The editors have asked me to broaden the context of discussion, and I shall refrain from closely examining each of the studies.

The growth of methodological interest in social mobility should not overwhelm theory. Obviously, mobility can be measured in many ways. Which way to go is not primarily a methodological question but an analytical one. I may be oversensitive but I fear that Laslett's fourth footnote seems to imply that the question of how to formulate interaction effects is a methodological rather than a theoretical issue.

Many sociologists have adopted a straightforward, rigorous—and likely unrewarding—mode of analysis. They are looking for a swift stream which carries conceptual gold. They then assay the quality of the gold by seeing how it does against dependent, usually attitudinal, variables. As it is panned through a wide variety of dependent variables, uneven or low-quality results emerge. In social mobility studies this style leads to concentration on the correlation between (additive or interactive) social positions, on one hand, and a dependent variable, on the other.

Consider a different approach. Overall correlations (even multiple or partial) are not the main concern. Rather, the target is to discover, within a group, the subgroups which are affected by the independent variable. For which Lutherans or Jews is status inconsistency important in regard to the dependent variables? How do those markedly affected by status inconsistency differ from fellow ethnics who seem unaffected by it?

The implicit model is one of detection, adding pieces together to gain a fuller picture, rather than one of locating the unique variable or variables which provide satisfying explanations. This alternative style is largely ignored in current sociological investigations. Its absence particularly mars evaluation studies which tend to ignore the question of which subgroups are particularly helped or not helped by a particular intervention. Rather, the focus is on obtaining a "yes/no" verdict in regard to an intervention (read independent variable); the diagnostic issue of what is important to whom is usually left unresolved.

The standard style also assumes the timelessness of its data. Studies of 1936 or 1952 have the same significance for 1971 as do studies of 1968. Implicitly there is a belief in the significance of structure regardless of changes in social climate. A pseudo-cumulativeness exists, acting as though data are floating in statistical space unconnected to their historical emergence. Laslett concludes by recognizing that individuals change over time. But even more than that, awareness is needed: social values also change over time.

None of the three studies includes age as a variable. At one level, people are at different stages of their mobility patterns because of differences in age; thus, the age factor may confound results. For example, a fifty-five-year-old who was affected by the depression of the thirties is likely to have a different outlook on social mobility than a thirty-five-year-old whose work life has not been marred by severe depression.

Some engineers are now unemployed. Probably few will ever become engineers again Their occupational lives have been profoundly changed by the deliberate curtailing of military production and the deliberate effort in 1969–70 to slow down the economy. Similarly, the current academic recession and prospective demographic shifts will deeply and perhaps permanently change the career lines of many now in graduate school.

If blacks get good jobs as a result of political pressure as often as because of individual merit or striving, how will they react to the fact of occupational advance? Or if "opting out" is not thought to be the same as "failing out" or "copping out," how then will white middle-class youths respond to downward mobility?

These specific and concrete events and changing social attitudes affect the possibilities of mobility and reactions to it. But it is not the standard sociological style to pay attention to them as we focus on structure rather than events or processes. We should not assume when we study the impact of social mobility that we are dealing with invariant relationships.

My guess is that social mobility and status inconsistency will increasingly fail to explain much about attitudes. Sociological attention will wander away from it at the same time that social mobility will become an important objective of national and local policies. "Open admissions" will

become more widespread as a way of increasing the chances for educational mobility of youth from minority and white working-class families. A new third-tier educational system (the Swedes call it "recurrent education") is emerging which provides education and training at whatever point in an individual's lifetime he wants it and in a way appropriate to him at that point. Occupational upgrading (or intra-generational mobility) is a primary though not exclusive purpose. Indeed, the notion of dead-end jobs is challenged as ladders to higher-level jobs are constructed where formerly no connections existed. Federal funds are likely to be increasingly spent on encouraging in-plant promotions, and some unions are now amassing educational allotments through collective bargaining. These funds can be used for the general educational interests of members.

In short, upward mobility—both intra- and inter-generational—is becoming more and more regarded as a social right, at least in the sense that government should provide many more opportunities and ways of achieving it. If mobility does come closer to the position of an assured experience, then its impact may well change. For then the experience will be more common; much mobility may become horizontal rather than vertical as individuals shift jobs to increase their work satisfactions as much as for prestige advance. (This change would become more likely if wage and salary differentials decreased and/or income in the form of public services or subsidies increased—that is, if there were overall decreases in income inequality.) Moving up then would require less individual zeal; it may be more socially acceptable to reject social mobility opportunities rather than to accept them automatically (especially if counterculture values persist).

What I have just outlined is not inevitable. It may happen that way, and my hope is for a future somewhat like this picture: greater equality in income and status but with considerable horizontal mobility to liven up activities. Whether this vision is good or bad, likely or unlikely, is not the main issue for this discussion. Rather, I would stress the impact of broader situations on the mobility experience. Mobility or status inconsistency means different things to different people at different historical points. If mobility is relatively easy to achieve *if* one wants it, or if its achievement depends clearly on governmental policies, then the experience of it may be much less of a significant element in one's life or it may have a different kind of significance than when it is regarded as one's own unique achievement or failure.

The study of social mobility has been obsessively concerned with consequences rather than causes. I have long been puzzled by this and have speculated about the fate of sociologists were they forbidden to use the concepts of mobility to explain behavior. A surprising amount of sociological literature would stop. I now suspect that the attraction of mobility-as-an-explainer was that it served in the fifties to provide a criticism of

Eisenhower bourgeois society. It was a way of saying: Watch out for the prices paid in conforming to the American motif of onward and upward. In both the fifties and sixties, the thesis of status inconsistency as the propellant of radicalism, whether left or right, deflected attention from the content of the radicalism to the motivation of the radicals. Again, this perspective fitted into the outlook of those critical of American society, from a liberal but not radical stance.

I do not have a full account here but I feel certain that the social (and personal) values of liberal sociologists led to a preoccupation with the examination of status and made it easier to accept with little criticism the results of rather weak studies. We have been able, for example, to pursue social mobility studies despite the fact that there is sizable dispersion in income for a given occupation, even when detailed occupational coding is used. Similarly, we regard education as an important explainer of income when at least two-thirds of income variation is unexplained by education.

By contrast, we have not sought to explain why there is a high dispersion of occupational income or why the low explanatory power of education. For these findings contradict important beliefs about the order and rationality of our economic and social system. Economists also find it difficult to recognize and analyze data which contest the orderliness of the structure of society.

Future social mobility investigations, I think, should turn more to the question of what produces or retards mobility or particular groups. This interest will force sociologists to move to the intermingling of economic and social forces and to overcome their economic illiteracy. Even more difficult will be a shifting of sociological concern from the focus on attitudes —despite the continuing evidence of the shakiness and utility of attitudinal measurements—to the broader forces which shape relations, values, and experiences.

Social mobility and status inconsistency are being downgraded as all-purpose explanatory variables. They will be useful if they are employed less indiscriminately. In the three studies, they are tested to see if they help explain social participation, political attitudes, work satisfaction, social integration, and emotional adjustment. And should be used less ahistorically. The downgrading is to the good, I think. But social mobility as one measure of the performance of this society will continue to be important. Paradoxically, it may grow in political significance as its impact on attitudes lessens.

Anomie in the Ghetto: A Study of Neighborhood Type, Race, and Anomie[1]

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Three inner-city neighborhoods in varying stages of racial change were surveyed. Contrary to previous studies, it was found that the Negro ghetto exhibited lower anomie than two other areas which were undergoing racial transition. It is suggested that the pattern of social organization of the ghetto may foster a normative system which leads to feelings of relative well-being, reflected in low anomie scores.

A popular pastime of both civil rights activists and sociologists is the discussion of the powerlessness and hopelessness among residents of the American Negro ghetto. This view of the "disorganized" ghetto as a concomitant of urbanization marks a sociological tradition tracing back to Georg Simmel (Wolff 1950, pp. 409–23). There is, however, a sharp contrast between Simmel's portrait of the inner-city resident and the sharpened image presented by contemporary researchers (Gans 1962; Greer and Kube 1959). It was recently reported that Negro residents of middle-class neighborhoods express fewer feelings of "anomia" and "powerlessness" than do inhabitants of the ghetto (Bullough 1967). Bullough also concluded that both of the latter forms of alienation were strongly linked with "almost any kind of deprivation" and suggested that alienation is "a product of segregated living." The findings of the present study challenge the attribution of a high level of anomie to the residents of American Negro ghettos.

The word "ghetto" is stereotypic in that it evokes an image of a disorganized neighborhood which contains an overdose of all the pathologies associated with urbanization. If one asks a social scientist, "What is a ghetto?" he probably will elicit a description of an urban area including at least three elements: (1) A high concentration of lower-class Negroes, (2) a high level of social disorganization, and (3) a high level of anomie. The purpose of this paper is to explore these three elements in more detail, focusing particularly on the relationship of anomie to neighborhood context.

[1] I express my particular thanks to the editors and reviewers of this *Journal* who made thoughtful suggestions and gave encouragement on several earlier versions of this paper. In addition, the following persons made helpful contributions Charles Bonjean, James Cox, Harold Brown, Stephen Finner, Paul McFarlane, John McGrath, Michael Lalli, Susan Londergan, Frederick Parker, and David Schulz.

In reviewing the history of the concept of neighborhood in American sociology, a frequently quoted source is Louis Wirth. He suggests (1961, pp. 75–76) that the limits of the ecological processes of a given area are "contingent upon subsistence and competition," but that social and psychological phenomena within an area might be more strongly determined by "individual characteristics, such as attitudes, personalities, cultural forms, social organizations, and institutions." Directing his commentary specifically toward anomie, Wirth also reflects on "the transitory character of highly urbanized areas and . . . the lack of participation . . . which constitutes essentially the state of anomie" (1938). It is clear that Wirth places equal emphasis on contextual factors and individual social attributes in explaining the life-style of urbanites. Several subsequent studies confirm Wirth's assertion that people highly involved within formal organizations in an urban setting reveal low levels of anomie (Bell 1957, p. 109; Mizruchi 1964, p. 112).

A second theoretical linkage between anomie and neighborhood type derives from Lander's ecological analysis of delinquency in Baltimore. Lander found that as an area reached maximal racial heterogeneity—50 percent white and 50 percent Negro—the delinquency rate of the area approached a maximum (Lander 1954, pp. 62–65). Such neighborhoods are usually passing through the most critical phase of the invasion-succession cycle, the constant uprooting and resettling has a disruptive effect on neighborhood organization, bringing about increased anomie. Thus, Lander implied the presence of anomie by correlating an area's racial composition with its delinquency rate The second neighborhood attribute which Lander found to be closely associated with delinquency was a low proportion of "home ownership," interpreted also by Lander to indicate the relative instability of an area (in terms of anomie).[2]

Two different, although not entirely contradictory, lines of reasoning have been presented so far. One trend, best exemplified by Bullough, contrasts the anomie of Negroes living in a ghetto with that of Negroes who moved to integrated middle-class neighborhoods. Controlling for education, income, and sex, Bullough found the anomie level of Negroes remaining in the ghetto to be significantly higher than that of their peers who departed for integrated neighborhoods. Realizing the initiative involved in moving to an integrated neighborhood, such a mobile group

[2] Recently, Gordon reexamined the factor-analysis technique which Lander applied in his Baltimore study and concluded that "socioeconomic status" should have been identified as the factor with the highest loading, rather than neighborhood racial composition or home ownership (Gordon 1967) Gordon's criticism tends to support a "class" theory to explain delinquency, rather than Lander's anomie theory which focuses on neighborhood disorganization. One of the basic questions emerging from the Lander-Gordon controversy is what is the relative influence of socioeconomic status and neighborhood social organization on anomie?

might be expected to demonstrate a stability and a self-sufficiency which would be reflected in anomie scores lower than anomie for similar groups remaining in the ghetto. But what of the lower-class Negro who is taking part in a Negro "invasion" of a white working-class area?

The second line of reasoning flows from Lander's work (1954). To reiterate, as an area approaches maximal racial heterogeneity, the level of anomie will be maximal; hence, we would expect lower anomie in a racially stabilized ghetto than in a racially changing area, which possesses a less completely integrated social structure. This study explores the relative validity of the two preceding viewpoints—contrasting the levels of anomie in several types of urban areas.

METHOD

In 1969 interviews were conducted with a 20 percent systematic sample (645) of the household heads in three inner-city neighborhoods of a medium-sized city. Employing the Srole anomie scale (Srole 1956), this study concentrates on the explanation of anomie at two levels of analysis —the *neighborhood* and *individual*.[3] The neighborhood analysis identifies the impact of a given contextual setting upon the attitudes of its residents. The individual approach relates the social attributes of respondents (the standard socioeconomic indicators, as well as selected social participation measures) to their individual anomie scores. Sociological studies have rarely identified the interaction between social context and individual attributes, primarily because they have tended to employ either an ecological or a social-psychological approach (Blau 1960, p. 179; Coleman 1969, pp. 86–114).

Several writers suggest that covariance and multiple-regression techniques be employed in contextual analysis; these procedures test the assumption of a common slope (of a dependent variable with respect to an independent variable) across groups as opposed to different slopes within groups (Schuessler 1969, p. 231; Hauser 1970, pp. 661–62).

The present research employs a covariance analysis to detect neighborhood differences in the pattern of association between anomie and the independent variables. This procedure is augmented by the use of multiple correlations at four levels of aggregation: (1) correlations among measures for all individuals in the sample; (2) correlations for all whites; (3) correlations for all Negroes; and (4) correlations for Negroes by individual neighborhood. The covariance analysis permits an assessment of contextual effects, whereas the multiple-regression analysis explicates

[3] For conceptual clarity, the "Srole anomia scale" will be referred to as the "anomie scale."

the relative effect of the independent variables at the individual level (level 1) and within given levels of aggregation.

HYPOTHESES

The main purpose of the study is to test the null hypotheses which follow. The first two, the principal hypotheses, ask whether the mean anomie scores vary significantly (1) across neighborhoods by race, and (2) within neighborhood by race. The other two hypotheses also explore anomie across and within neighborhoods; each hypothesis incorporates a different series of "test" variables.

I. Anomie by Neighborhood

$$H_0: (a)\ \overline{X}_{1N} = \overline{X}_{2N} = \overline{X}_{3N},$$
$$(b)\ \overline{X}_{2W} = \overline{X}_{3W}.$$

Hypothesis I states. (*a*) the mean anomie scores of Negroes are the same in each of the three neighborhoods; (*b*) the mean anomie scores of whites are the same in neighborhoods 2 and 3.[4]

II. Negro versus White Anomie

$$H_0: \overline{X}_{1N} = \overline{X}_{1W}, \overline{X}_{2N} = \overline{X}_{2W}, \overline{X}_{3N} = \overline{X}_{3W}$$

Hypothesis II states that the mean anomie score for Negroes in each of the neighborhoods equals that for whites.[5]

III. Socioeconomic Status

A. Occupation

$$H_0: (a)\ b_1 = b_2 = b_3;$$
$$(b)\ \overline{H} \leqslant \overline{B} \leqslant \overline{W} \text{ (for Negroes by neighborhood and all whites).}$$

[4] Lander (1954, pp 62–65) implies that differing levels of anomie in inner-city areas are a function of the racial composition of neighborhoods Bell (p 108) found neighborhood differences in anomie scores to be linked to the "economic" and "family" status of urban areas The present hypothesis suggests the existence of neighborhood differences in anomie scores, even though all three areas are of "low" economic status Following Lander's reasoning, the mean anomie score in the Ghetto would be lower than in the other two areas Whites of neighborhood 1 (Ghetto) are excluded from the analysis because of insufficient sample size

[5] Middleton (1963, p 975), studying a Florida city, found that Negroes scored higher than whites on a scale of general alienation, however, no neighborhood comparisons were made Killian and Grigg (1962, p 664), indicated that urban Negroes did not differ significantly from whites in anomie Although previous research is inconclusive, Hypothesis II follows the reasoning expressed by Merton (1963, pp 161–94) in his "means-ends paradigm". The overall position of Negroes vis-à-vis whites in the total society gives rise to the expression of greater anomie. The hypothesis thus is that Negroes will exhibit higher anomie than whites in any neighborhood.

Hypothesis *A* states: (*a*) the slopes of anomie *y* on occupation *x* for the three neighborhoods are equal (i.e., the three neighborhoods have a common slope); (*b*) within each neighborhood (and for all whites) the mean anomie score of the "homebound"[6] is equal to or smaller than that of "blue-collar" workers, which is in turn equal to or smaller than that of white-collar workers.[7]

B. Education

H_0: (*a*) $b_1 = b_2 = b_3$;
(*b*) $\overline{\text{Lo.Ed.}} \leqslant \overline{\text{Hi.Ed.}}$ (for Negroes by neighborhood and for all whites).

Hypothesis *B* states: (*a*) the slopes of anomie *y* on education *x* for the three neighborhoods are equal; (*b*) the mean anomie score of those with low education is equal to or smaller than for those with high education.[8]

C. Home Ownership

H_0: (*a*) $b_1 = b_2 = b_3$;
(*b*) $\overline{R} \leqslant \overline{O}$ (for Negroes by neighborhood and all whites).

Hypothesis *C* states: (*a*) the general covariance proposition with respect to anomie and home ownership; (*b*) the mean anomie scores for renters will be lower than or equal to that of home owners.[9]

[6] "Homebound" is a residual category of household heads, those who are not members of the labor force—mainly housewives, retired persons, and disabled persons

[7] In general, the evidence indicates the higher the SES of the respondent, the lower his anomie score (Srole 1956, p 715; Mizruchi 1964, pp 91–121; Greer and Kube 1959, p. 97). Bell (1957, p 113) found that for both the *neighborhood* and the *individual*, the higher the SES, the lower was the anomie score of respondents. Although most studies have combined indices of education and occupation into a general measure of SES, the general expectation is that persons in "white-collar" occupations will express less anomie than those with blue-collar status A glaring exception appeared in an early replication of the original Srole study by Roberts and Rokeach (1956), they found that anomie was *not* related significantly to occupational status, although there was a strong negative correlation between higher education and anomie. Following the general pattern set by these several studies, however, it is hypothesized that anomie is correlated negatively with all three SES measures—occupation, education, and home ownership—and that even with covariance controls for SES, neighborhood differences in anomie will occur.

[8] See n. 7 for an explanation of this hypothesis.

[9] Lander (1954, pp 62–82) concluded that proportion of home owners in an area is the most powerful statistical predictor of delinquency, and further that home ownership was a measure of social stability or anomie (p 82) Gordon (1967, pp 938–39), in his reanalysis of the data, suggested that Lander placed too much emphasis on the face validity of his interpretation Rather than being a measure of the social stability of an area, Gordon argued, owner occupancy might better have been interpreted as a

IV. Social Integration

A. Length of Residence

H_0: (*a*) $b_1 = b_2 = b_3$;
(*b*) $\overline{T_1} \leqslant \overline{T_2} \leqslant \overline{T_3} \leqslant \overline{T_4}$ (for Negroes by neighborhood and all whites).

Hypothesis *A* states: (*a*) the general covariance relationship for anomie and the length of time which a person has resided in a neighborhood; (*b*) the mean anomie score for persons who have resided in a neighborhood for a shorter period of time is lower than or equal to the mean score of those who have lived there longer.[10]

B. Social Participation

H_0: (*a*) $b_1 = b_2 = b_3$;
(*b*) $\overline{\text{HiP}} \geqslant \overline{\text{LoP}}$ (for Negroes by neighborhood and all whites).

Hypothesis *B* states: (*a*) the covariance proposition regarding anomie score and the Chapin social participation scale (Chapin 1955, pp. 275–78); (*b*) that individuals who have high social-participation scores have at least as high or higher anomie scores than those who are lower in social participation.[11]

C. Neighborliness

H_0: (*a*) $b_1 = b_2 = b_3$;
(*b*) $\overline{\text{Very HiN}} \geqslant \overline{\text{HiN}} \geqslant \overline{\text{ModN}} \geqslant \overline{\text{LoN}}$ (for Negroes by neighborhood and all whites).

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measure of a population's capacity to move where it wants Following Gordon's logic, home ownership is classified as an SES factor in the present study, rather than as a social-integration factor The hypotheses suggest that anomie is negatively correlated with home ownership, but that controlling for home ownership will not destroy the originally hypothesized differences in neighborhood anomie levels

[10] Although there is a dearth of empirical findings about the relationship between length of time a person has lived in a neighborhood and his anomie score, it has been suggested that the range of contacts and the number of friendships within a neighborhood are a positive function of time (Keller 1968, p 79). These factors of social integration, in turn, should be associated negatively with anomie Thus, those who have lived in a neighborhood longest should manifest relatively low anomie It is hypothesized also that controlling for time will not eliminate the contextual effects of neighborhood

[11] If anomie is indicative of the absence of integration within a social system, then persons who are less active in formal organizations should manifest higher anomie than do the active Several studies relating social participation to anomie have verified this assumption (Bell 1957, pp 109–11; Mizruchi 1960, pp 647–54) A negative association between anomie and participation is hypothesized in the present study, although it is expected that the covariance procedure will not neutralize the effect of different neighborhood contexts.

Hypothesis *C* states: (*a*) the covariance hypothesis for anomie and neighborliness scores, as measured by the Wallin neighborliness scale (Wallin 1953); (*b*) that persons with "higher" neighborliness scores have at least as high or higher anomie scores than those with "high" neighborliness scores, etc.[12]

NEIGHBORHOOD PROFILES

The study was carried out in a city of 83,000 persons located in the eastern megalopolis, and is the center of a Standard Metropolitan Statistical Area of approximately 500,000 population. The area studied is undergoing racial invasion and succession, with the result that there is an increasingly black-core city, surrounded by an expanding belt of white suburbs, enveloping what was formerly rich farm land. Neither the state nor the city had an effective policy on housing at the time of the study, nor were there any major changes in Negro migration within the city other than the normal continuation of the pattern of expansion of the black core. All three of the areas studied were low-income neighborhoods, with median household incomes under $6,000. None had undergone any community conflicts or civil unrest. (The general population characteristics of the three areas appear in table 1.)

Ghetto

Ghetto has the highest concentration of Negroes in the city and is the city's oldest Negro neighborhood; its Negro population replaced a Polish-American population during World War II. The Polish population previously had displaced a German population which arrived in the city during the late 1800s and early 1900s. Ghetto is geographically well defined by a creek on one side, a high-speed rail line on another, and major highways on the other two sides. It is served by several Negro churches, settlement houses, store-front social service centers, a YWCA, and a YMCA, all of which might be expected to sustain both formal and informal social ties. Ghetto seems to function with a minimum of conflict—both internally and with outside white institutions—and to have a high degree of internal leadership. However, when compared with the other

12 The Wallin neighborliness scale purports to measure both intensity of interaction with neighbors and degree of positive sentiment toward neighbors (see Fava 1956; Greer 1956 for applications of this scale) Although I know of no instance where Wallin's scale has been used in conjunction with the Srole anomie scale, it is incorporated here as a general indicator of the intensity of neighboring within a given area. Hence, the research hypothesis is that scores for neighboring, like other social participation indices, will be negatively associated with anomie. Second, it is hypothesized that neighborhood differences in anomie will persist when neighboring is controlled by the covariance analysis.

TABLE 1

POPULATION CHARACTERISTICS OF THREE INNER-CITY NEIGHBORHOODS, 1969

CHARACTERISTIC	NEIGHBORHOOD		
	Ghetto	Northeast	West Side
Total population	2,558	3,325	4,715
Percentage Negro	96	85	63
Adult unemployment rate (time of study)	12	11	13
Percentage of adults with less than 12 years of education	62	49	65
Percentage of school population (age 16 years and above) dropped out of public schools during study year	40	12	16
Percentage of juveniles 7–17 years referred to courts during study year			
White males	*	*	14
White females	*	*	4
Nonwhite males	36	17	16
Nonwhite females	9	7	6
Dependency.			
Percentage of families receiving AFDC at time of study	34	20	24
Illegitimacy			
Percentage of illegitimate births during 1969	65	43	32

* Insufficient number of white juveniles to compute juvenile delinquency rate

two neighborhoods, Ghetto shows a higher prevalence of social pathology. percentage of school dropouts, juvenile delinquency, illegitimacy, and dependency (percentage of families in the area receiving Aid to Families with Dependent Children).[13]

Northeast

Northeast area might also be classified as a ghetto, but certainly not in the extreme sense as Ghetto. Although Northeast seems to be a working-class neighborhood by economic criteria, it is lacking visibly in local citizens' organizations, churches, settlement houses, and other supportive structures which might serve to integrate social ties. Between 1960 and the year of the study (1969), this area's racial composition shifted abruptly from 15 to 85 percent Negro. Northeast also evinces a high level of unemployment, school dropout, delinquency, and dependency, although not as high as Ghetto.

West Side

The third area, West Side, approximates the prototype of Lander's area of "maximal racial heterogeneity"—63 percent Negro and 37 percent

[13] The fundamental demographic data employed in computing these rates were derived from the 1969 sample survey, while data on school dropouts, delinquency, illegitimacy, and dependency were collected from agencies

white. West Side was in the most rapid state of racial transition of the three areas, shifting from an Italian-American working-class neighborhood to a racially integrated lower-class area.[14] Like Northeast, West Side seems also to lack viable recreational, religious, and civic organizations, particularly for its Negro population. It has the highest unemployment rate of the three neighborhoods, along with a high level of dependency and illegitimacy.

RESULTS

Results are presented in two parts which correspond to the major sections of the study: (1) covariance analysis of anomie scores and each of the independent variables and (2) zero-order and multiple correlations within the four previously stated levels of aggregation.

I. Anomie by Neighborhood

Table 2 presents the mean anomie scores for Negroes and whites by neighborhood. Negro scores are significantly lower in Ghetto than in the other two neighborhoods, whereas white scores do not differ significantly between the two nonghetto neighborhoods. Only in Ghetto is there a significant difference in anomie between whites and Negroes, whites expressing higher anomie.[15] Thus, for the Negro populations of all three neighborhoods Lander's theory is supported—anomie is highest when the

TABLE 2

MEAN ANOMIE SCORES AMONG HOUSEHOLD HEADS OF THREE INNER-CITY NEIGHBORHOODS

	Ghetto (96% Negro)			Northeast (85% Negro)			West Side (63% Negro)			Variance*	
Race	Mean	SD	N	Mean	SD	N	Mean	SD	N	F	P
Negro ...	8.7	2.8	(203)	9.8	3.2	(148)	10.4	3.2	(132)	13.60	< .001
White ...	10.3	3.0	(6)	9.1	3.1	(47)	9.7	3.1	(109)	1.83	> .20

NOTE.—$t = 3.83$, $P < .01$; $t = 1.28$, $P > .20$, $t = 1.30$, $P > .10$

* Analysis of variance computed for whites of North East and West Side only because of the small number of whites in Ghetto sample

14 Approximately seven out of ten of the Negro households have moved into the West Side within the past five years, as compared to about three out of ten of the whites.

15 Even this difference must be considered tentative because of the small number of whites in the Ghetto sample ($N = 6$) A 1966 study of the same three neighborhoods did reveal a similar pattern. lowest Negro anomie in Ghetto, an intermediate level in Northeast, and highest anomie in West Side (Wilson 1968, p 12). Whites residing in Ghetto, as in the present study (1969), expressed higher anomie than whites of the other two areas. The reader should also note that although the range of the Srole

racial mixture approaches 50 percent white and 50 percent Negro. The theory is not supported for whites, however.[16]

II. Anomie, SES, and Neighborhood

Table 3 presents an analysis of anomie by occupational status, education, and home ownership, together with the covariance analysis across neighborhoods (and *t*-tests within each neighborhood).[17] The three covariance tests indicate the absence of a common slope across the three neighborhoods, revealing an interaction between neighborhood context and

TABLE 3

MEAN ANOMIE SCORES OF HOUSEHOLD HEADS BY NEIGHBORHOOD, OCCUPATIONAL STATUS, EDUCATION, AND HOME OWNERSHIP

	WHITE	NEGRO			COVARIANCE*	
MEASURE	Three Neighborhoods Combined	Ghetto (96% Negro)	Northeast (85% Negro)	West Side (63% Negro)	*F*	*P*<
Occupational status.						
(a) White collar . .	9 0 (27)	8 7 (18)	10 2 (20)	10 8 (6)	11 88	001
(b) Blue collar	9 6 (59)	8 6 (120)	10 1 (106)	10 5 (102)		
(c) Home bound†	9 9 (76)	9 4 (64)	8 3 (20)	10 3 (23)		
Education						
(d) 12 years and over	8 9 (47)	8 5 (64)	9 3 (57)	9.9 (40)	92 17	001
(e) Under 12 years .	9 9 (115)	9 0 (135)	10 5 (91)	10 6 (89)		
Home Ownership						
(f) Owner . ..	9 0 (112)	8 2 (112)	9 9 (109)	10 1 (72)	11 76	001
(g) Renter	10 6 (50)	9 8 (88)	9 1 (39)	10 8 (60)		
	T-Tests (1-Tailed for Null Hypothesis Tests)					
Comparison.						
a-b	N S.	N S.	N S			
b-c	N S	$P < 10$	N S ‡	N S		
a-c	N S.	N S	N S	N.S		
d-e	$P < .025$	N S	$P < 05$	N S		
f-g	$P < 001$	$P < 001$	N S	$P < 10$		

* Covariance computed across neighborhoods, whites excluded
† "Home Bound" refers to those not included in the labor force housewives, retired persons and disabled persons
‡ If 2-tailed hypothesis had been tested, *t*-test would have been significant at 05 level

scale varies from 5 to 15, the average standard deviation throughout this research is approximately 3 Thus, the typical "anomic" respondent differs from the typical "nonanomic" respondent in that the former (but not the latter) agrees with the following two statements (1) "Nowadays a person has to live pretty much for today and let tomorrow take care of itself," and (2) "In spite of what some people say, the lot of the average man is getting worse, not better"

[16] These findings are in direct contradiction with Bullough's contention (1967, p 477) that alienation "is a product of segregated living"

[17] Whites are not included in the covariance analysis because of an insufficient number of respondents in Ghetto to insure statistical reliability

anomie when each of the SES factors is controlled. If one examines the within-neighborhood comparisons, in no instance do the anomie scores of white-collar workers differ significantly from those of blue-collar workers. In Northeast the "homebound" are significantly lower in anomie than are the white-collar and blue-collar workers, whereas in Ghetto the homebound have higher anomie scores than the blue-collar group. In general, the within-group occupational variable appears to be more illuminating in terms of its *lack* of predictive utility.

Educational level is also related inconsistently to anomie, lower education being associated with anomie in only one neighborhood (Northeast) and among whites. Home owners, among whites, manifest less anomie than do renters. The same relationship of home ownership and lower anomie is evident in two of the neighborhoods (Ghetto and West Side), although there is no significant difference between scores of owners and renters in the third area (Northeast).

III. Anomie and Length of Residence in Neighborhood

Table 4 relates mean anomie score and the length of residence in a given neighborhood, and again the F-ratio indicates the absence of a common slope across the three neighborhoods. Anomie, then, is not a simple function of increasing exposure to a given residential context. Although the mean anomie score of each racial group decreases as its members live in a neighborhood for a decade or longer, the regression line is rather un-

TABLE 4

MEAN ANOMIE SCORES OF HOUSEHOLD HEADS BY NEIGHBORHOOD AND LENGTH OF RESIDENCE IN NEIGHBORHOOD

Length of Residence	White: Three Neighborhoods Combined	Negro: Ghetto (96% Negro)	Negro: Northeast (85% Negro)	Negro: West Side (63% Negro)	Covariance*: F	Covariance*: $P<$
(a) Under 2 years	10 0 (34)	9 6 (20)	9 9 (30)	10 8 (41)	11 67	001
(b) 2–4 years	11 1 (19)	8 4 (29)	10.4 (20)	10 2 (31)		
(c) 5–9 years	9 8 (26)	9 0 (46)	10 1 (59)	10 6 (43)		
(d) 10 years and over	9 0 (83)	8.6 (108)	9 0 (38)	9 2 (17)		
	T-Tests (1-Tailed for Null Hypothesis Tests)					
Comparison:						
a-b	N S.	N S.	N S	N S		
b-c	$P < .10$	N S	N S	N S.		
c-d	N S.	N S	$P <$ 05	$P <$ 10		
a-c	N S.	N S.	N.S.	N S.		
b-d	$P <$ 01	N S	$P <$ 025	N S.		
a-d	$P < .10$	$P < .10$	$P <$ 10	$P <$ 025		

* Covariance computed across neighborhoods, whites excluded.

even; those who have lived in the neighborhood for an intermediate number of years reveal higher anomie scores than those who have lived there for a relatively short period. This irregularity in the middle time range is most evident among whites, and among Negroes of the Northeast area. These diverse patterns suggest that the high anomie scores of Northeast and West Side Negroes are not merely manifestations of the temporary strain of adjustment upon moving to a new neighborhood. Rather, the varying slopes suggest again that each neighborhood has its distinctive contextual effect throughout the whole time-range.

This evidence does not, however, preclude the differences in anomie being due to a "selective migration." If persons sharing a certain outlook on life are drawn to a specific neighborhood because of a preconceived image of the area, differences in anomie then may have originated long before the move occurred. Although a hypothesis of selective migration is plausible for many Negroes, it is untenable for most whites, the majority of whom moved to their neighborhood long before it became predominantly black. Despite the supposedly unsettling experience of racial invasion, whites who have lived in a neighborhood for ten years or more reveal significantly less anomie than do those who have resided there for shorter periods. Although whites who have remained in an area throughout the racial transition express relatively low anomie, it is possible that the more anomic whites fled from their neighborhoods during the earlier phases of racial invasion, leaving the satisfied (nonanomics) to face the full impact of integration.

IV. Anomie, Social Participation, and Neighborliness

Table 5 relates anomie to social participation and to neighborliness, that is, to both participation in formal organizations and to informal interaction among neighbors.[18] Again, for both independent variables, the covariance analysis testifies to the existence of neighborhood contextual effects. The three neighborhoods reveal vastly different patterns of association between anomie and the two modes of social interaction. Anomie in Ghetto is associated positively with neighborliness, but is associated negatively with participation in formal organizations. In direct contrast, Northeast Negroes involved in organizations reveal higher anomie scores than nonparticipants. Neighborliness in the Northeast appears to be correlated with anomie in a curvilinear pattern, in that those who are both "very high" and "low" in neighborliness display high levels of anomie.

[18] Scores on Chapin's social participation scale are dichotomized (high and low) because of the relatively small proportion of respondents participating in any formal organization; "high" participation indicates Chapin scores of 1–99, while "low" indicates a score of 0.

TABLE 5

MEAN ANOMIE SCORES OF HOUSEHOLD HEADS BY SOCIAL PARTICIPATION AND NEIGHBORLINESS SCORES

	WHITE	NEGRO			COVARIANCE*	
MEASURE	Three Neighborhoods Combined	Ghetto (96% Negro)	Northeast (85% Negro)	West Side (63% Negro)	F	$P<$
Social participation score:						
(a) High	10.0 (42)	8 2 (54)	10 9 (44)	11 2 (45)	3 62	05
(b) Low	9.4 (120)	8 9 (149)	9 4 (103)	10 0 (87)		
Neighborliness score.						
(c) 11–13 (very high)	7 6 (31)	9 9 (34)	10 9 (26)	9 5 (35)	13 16	001
(d) 8–10 (high) ...	9 1 (27)	8 7 (51)	9 8 (38)	10 8 (22)		
(e) 5–7 (moderate)	9.9 (40)	8 5 (57)	8 6 (46)	10.6 (29)		
(f) 0–4 (low) .	10 6 (64)	8 5 (61)	10 5 (36)	10 9 (46)		
	T-Tests (1-Tailed for Null Hypothesis Tests)					
Comparison:						
a-b	N S †	$P<$ 10	N S †	N S †		
c-d	$P<$ 05	N S.†	N S	N S.†		
d-e	N S	N S.	N S †	N S		
e-f	$P<$ 10	N S	$P<$ 01	N S.		
c-e	$P<$ .01	N S †	N S.†	N S.†		
d-f	$P<$ 025	N S.	N S.	N S		
c-f	$P<$ 001	N.S.†	N S.	N S †		

* Covariance computed across neighborhoods, whites excluded

† If 2-tailed hypothesis had been tested, t-test would have been significant at 05 level

(Those who fall in the intermediate range of the neighborliness scale, however, have lower anomie scores.) Still a third pattern is displayed by West Side respondents—here social participation is linked with high anomie, while neighborliness is significantly related to low anomie. Finally, among whites, neighborliness follows an almost uniformly linear pattern, an inverse association with anomie over the whole scale; while social participation is associated positively with anomie.

Hypotheses Reviewed

First, all of the null hypotheses stating that the three neighborhood slopes are equal ($b_1 = b_2 = b_3$) are rejected; factors internal to each neighborhood account for more of the variance in anomie than do individual socioeconomic status, length of residence, or patterns of formal participation and neighborliness. Second, the hypothesis that Negro anomie scores do not vary across neighborhoods is rejected; that white scores do not differ across neighborhoods is not rejected, however. Third, the null hypothesis of no significant difference between white and Negro anomie scores is rejected only for Ghetto. Fourth, the null hypotheses linking anomie to SES (occupation, education, and home ownership) are

rejected inconsistently, failing to apply uniformly for all three areas and for whites. Fifth, the null hypothesis relating anomie to length of residence is not rejected in its general form, although it is rejected consistently when new residents are compared with those who have lived in an area more than ten years. Last, the two hypotheses tying anomie to specific participation and to neighborliness patterns are not rejected in their comprehensive form, but are rejected in several instances for individual neighborhoods. Thus, the covariance tests suggest an underlying relationship between neighborhood context and anomie. In the next section, zero-order and multiple correlations at several levels of aggregation are explored with the aim of unraveling the interaction between individual attributes and anomie.

Correlations for Individuals

The zero-order correlations between anomie and the ten independent variables are summarized in table 6. Three variables, age, sex, and household size, which have been found to be associated with anomie in "social area analysis" are included also as test variables (Bell 1957, pp. 105–15; Greer and Kube 1959, pp. 97-100).[19]

Most of the correlations are low or intermediate, indicating that although these independent variables have explanatory power, they do not embrace a total social system. An explanation for the low correlations at the higher levels of aggregation is immediately evident from the covariance results which revealed highly diverse neighborhood effects. By computing zero-order correlations at levels of aggregation above the neighborhood, we have in several instances averaged a series of neighborhood-level correlations of varying strength and direction, resulting in uniformly low correlations. That correlations are low for all four levels of aggregation may be due also to the demographic homogeneity of the respondents. Selecting a sample from three low-income areas is a form of control procedure that precludes much of the variance (in both independent and dependent variables) which might have been present in a more heterogeneous sample of neighborhoods It should be emphasized, nevertheless,

[19] Sex is included as a dummy variable (male = 0, female = 1). Occupation is also conceptualized as a dummy variable (homebound and blue collar = 0; white collar = 1), as is ethnicity (Negro = 0, white = 1) and home ownership (rent = 0, own = 1). All of the remaining independent variables are actually ordinal variables which are treated as though they were interval measures For a general discussion of ordinal and dummy variables in multiple regression, their legitimacy and their applicability, see Cohen (1968) and Boyle (1966). For a discussion of dummy variables and neighborhood context, see Sewell and Armer (1966, pp 166–68) In an alternate regression analysis, occupation was processed as an ordinal variable, (i e, 1 = homebound, 2 = blue collar, 3 = white collar), but no significant difference was found in the amount of variance explained

TABLE 6

ZERO-ORDER CORRELATIONS BETWEEN ANOMIE SCORE AND TEN INDEPENDENT VARIABLES AT FOUR LEVELS OF AGGREGATION. TOTAL SAMPLE, ALL WHITES, ALL NEGROES, NEGROES BY NEIGHBORHOOD

Level	Ethnicity	Age	Sex	Household Size	Home Ownership	Education	Occupation	Social Participation	Neighborliness	Time in Neighborhood
Total sample	—03	03	02	13*	—13*	—13*	—03	06	—06	—20*
All whites		06	09	05	—19*	—16*	—13	—01	—34*	—16*
All Negroes		01	—01	16*	—11*	—12*	01	09*	03	—23*
Negroes:										
Ghetto		08	09	08	—24*	—17*	—08	—10	18*	—12
Northeast		13	—08	14	05	—13	12	22*	04	—13
West Side		07	04	04	—13	—11	—05	12	—13	—21*

* Designated correlation is significant at the 05 level

that these are individual correlations, rather than ecological correlations; the latter would be characteristically higher (with the unit of analysis being the aggregate or area).

Table 6 shows education is negatively correlated with anomie at all four levels of aggregation. Occupational status, however, is uncorrelated with anomie at any level of aggregation. (This may reflect the fact that the range of occupations in the sample is constricted, with no upper-level professional or managerial professions reported.) In contrast with the traditional SES variables, both home ownership and length of residence show significant negative correlations with anomie, the former being the strongest correlation of the entire series. This outcome suggests that there is a high level of social integration among those who are firmly entrenched in an area (i.e., by buying a house or by residing in the neighborhood for a relatively long period). Anomie is also positively correlated with household size; in these low-income areas children are not only a financial liability, but probably a psychological hindrance as well (Peterson 1969, pp. 509–12).

The two measures of social interaction (social participation and neighborliness) exhibit wide neighborhood variations in their degree of correlation with anomie. The high anomie scores of organizational participants in Northeast and West Side may not represent organizational membership per se, so much as they reflect an anxious quest for "mediating" ties in a transitional neighborhood (Neal and Seeman 1964, pp. 216–26). The low anomie of organizational participants in Ghetto, by contrast, may indicate that formal organizations in older and established areas do provide effective linkages with the neighborhood social structure (Litwak 1961; Gutman 1963, p. 176).

The second measure of social interaction (neighborliness) shows a positive correlation with anomie in Ghetto. The other two neighborhoods, in contrast, reveal correlations between anomie and neighborliness which are highly variable and unpredictable. Keller (1968, pp. 46–47) indicates this erratic pattern is typical of transitional areas. A third pattern, which is displayed among whites of the three areas, shows a moderate negative correlation between anomie and neighborliness, suggesting that (for whites) neighboring may serve as an integrative process in a neighborhood populated mainly by Negroes.

Contrary to the original hypothesis, then, Ghetto residents who are "very high" in neighborliness are also high in anomie. Unlike respondents with lower neighborliness scores, those with "very high" scores indicate: (1) that they have been inside of more than four of their neighbors' homes; (2) that they have attended movies, picnics, clubs, or other events with their neighbors; and (3) that they have exchanged or borrowed articles from their neighbors. (These three items of the neighborli-

ness scale measure intensity of interaction with neighbors, as distinct from the other items, which measure friendliness or positive sentiments.) Hence, low anomie is found among Ghetto residents who restrict their contacts with neighbors, again confirming a normative pattern typical of "older" neighborhoods, where the dominant neighboring norms may be characterized as guarded, selective, and noncommittal (Keller 1968, pp. 46–47). In a poverty-stricken ghetto the intensity of neighborliness may be constrained even further by a well-founded fear of various types of exploitive behavior (Rainwater 1970, pp. 371–73).[20] Thus, anomie appears to be a function of norms which govern neighborliness and social participation at various stages of neighborhood development. As the invasion and succession process ebbs in the two transitional areas (Northeast and West Side), a decrease in the level of anomie would be predicted. This trend would be marked by changes in both neighborliness and social participation norms. Rather than continuing its initial "mediating" function, social participation would serve as an ongoing link with the neighborhood and community. Neighborliness, rather than carrying on its original exploratory function, would be limited to a modicum of mutual aid and occasional visiting. In short, the pattern of social interaction of these two areas would approximate the one which was found originally in Ghetto. In all three areas anomie would be associated positively with neighborliness and associated negatively with social participation.

Table 7 summarizes the results of the multiple-regression analysis at

TABLE 7

PERCENTAGE OF VARIANCE IN ANOMIE SCORES EXPLAINED BY INDEPENDENT VARIABLES AT FOUR LEVELS OF AGGREGATION TOTAL SAMPLE, ALL WHITES, ALL NEGROES, NEGROES BY NEIGHBORHOOD

Combination of Independent Variables	Total Sample (R^2)	All Whites (R^2)	All Negroes (R^2)	Ghetto Negroes* (R^2)	Northeast Negroes* (R^2)	West Side Negroes* (R^2)
All variables (1, 2, 3, 4)	12 2	18.7	13 5	18.0	12 8	10 1
1, 2, 3	10 1	16.4	11 9	14 6	8 8	7 5
1, 2	7 6	12.1	9 3	6 3	5 9	6 3
1 (raw)†	5.7	0 1	7 6			
1 (net)‡	4 9	0.1	6 7			

NOTE Dictionary of variables 1, neighborhood of residence, 2, social integration factors (social participation, neighborliness, time-in-neighborhood) 3, SES factors (home ownership, education, occupation), 4, other social factors (ethnicity, age, sex, household size)

* Neighborhood of residence is excluded from within-neighborhood regressions (Ghetto, Northeast, and West Side)

† The variance indicated is the "raw" (zero-order) variance

‡ The variance indicated is the "net" variance, controlling for factors 2, 3, and 4

[20] This absence of "manifest" or overt neighborliness, however, does not necessarily imply a lack of social cohesiveness in Ghetto (Mann 1954, p 168). The social solidarity of a neighborhood may inhere also in "latent" neighborliness—that is, neighborliness may be characterized principally by positive attitudes that result in action mainly in times of crisis or urgent need

four levels of aggregation. Here "neighborhood of residence" and anomie are treated as "dummy" variables.[21] The remaining variables are grouped under three headings: (1) social integration factors, (social participation, neighborliness, and length of residence); (2) SES factors, (home ownership, education, and occupation—although following Lander, home ownership might be more properly included under [1]); and (3) other social factors (age, sex, and household size). Because the presence of contextual effects has been established through the analysis of covariance, it is important to estimate the proportion of variance explained by neighborhood context when all eleven independent variables are considered simultaneously. The left column of table 7 for the first regression includes all eleven independent variables, with each successive regression eliminating one category of variables. In the second regression, "other social factors" are excluded, and in the third, "SES Factors" are removed, etc.[22] Both "raw" and "net" variances are reported for "neighborhood of residence." The former represents the zero-order variance when "neighborhood" is entered as the first variable in a stepwise regression. The latter represents the variance associated with a standardized partial correlation between anomie and neighborhood, controlling for the other independent variables. It is this "net" variance which is referred to in the following discussion.

"Neighborhood of residence" accounts for approximately 5 percent of the variance in anomie at the "Total Sample" level, 7 percent for "All Negroes," but less than 1 percent for "All Whites." While "neighborhood" and "social integration" factors together account for about 12 percent of the variance in the anomie scores of whites, when this combined variance is compared with the "raw" (zero-order) variance for "neighborhood," it is found that the social integration factors alone represent almost the entire 12 percent. In a similar comparison for Negroes, however, the reverse relationship is found. The "neighborhood" variance alone accounts for more than half of the combined (neighborhood and social integration) variance. In contrast, the three within-neighborhood regressions (in which the "neighborhood" variable is eliminated) show that the social integration

[21] The dummy variables indicating the neighborhoods were coded as follows $X_1 = 1$ if respondent resided in the Northeast, and 0 if otherwise; and $X_2 = 1$ if respondent resided in the West Side, and 0 if otherwise. Because of the "nonsingularity" constraint, residence in Ghetto was treated as a reference point Ghetto respondents received a zero-code for both X_1 and X_2 Anomie was also treated as a dummy variable, dichotomizing into "high" (11–15) and "low" (5–10) scores This procedure was found to "smooth out" some of the curvilinearity displayed in covariance results

[22] This procedure, based upon the work of Blau and Duncan (1967, 128–47) eliminates many of the problems associated with single stepwise regressions when a series of independent variables are highly intercorrelated, as is often the case in sociology In the present research a limited number of combinations of independent variables are employed, thereby following Gordon's prescription to beware of "partialling" without adequate theoretical grounds for doing so (1968, pp 593–95)

factors explain consistently about 6 percent of the variance in anomie. This increase in the "social integration" variance at the neighborhood level reflects the fact that two of the social integration measures (social participation and neighborliness) vary substantially across neighborhoods in strength and direction. In the regressions at higher levels than the neighborhood, in which neighborhood is treated as a dummy variable, part of the "social integration" variance is absorbed by the neighborhood variable. Hence, both the multiple regression and covariance analyses lead to the same conclusion—for Negroes, anomie is influenced strongly by an interaction between social integration factors and neighborhood context, the latter being the stronger effect; for whites, there is a minimum of interaction between social integration factors and neighborhood, although social integration factors do exert a moderate influence on anomie. Thus, among Negroes neighborhood context appears to be a dominant factor affecting anomie, but for whites the overwhelming social fact may be that of being white in an area which is predominantly black. From this evidence, it not only seems clear that "neighborhood" has utility as a general explanatory variable, such as age or education, but that in some instances (i.e., for Negroes) neighborhood appears to have a statistical power approaching that of the traditional sociological variables.[23] Finally, a pattern which is found consistently in this research is that neighborhood, combined with social integration factors, accounts for approximately half of the explained variance in anomie.[24] These results lead to a reevaluation of the "relative deprivation" explanation of anomie, which has typically placed greatest emphasis on the SES variables.

SUMMARY AND DISCUSSION

Ghetto revealed the highest incidence of social pathology among the three inner-city neighborhoods (the highest rate of delinquency, dependency, illegitimacy, and high school dropouts), but displayed the least anomie. Among Negroes, the differences in anomie appeared to be more strongly associated with differing neighborhood contexts than with the separate effects of individuals' socioeconomic status, length of time in neighborhood, social participation, or neighborliness.

In only one neighborhood, Ghetto, did Negro and white anomie scores differ significantly, Negroes manifesting lower scores. (Even this difference

[23] Schuman and Gruenberg recently demonstrated that "city of residence" may be employed in a similar manner, as an independent variable in explaining racial attitudes (1970).

[24] Controlling for both "SES factors" and "other social factors," the percentage of variance in anomie explained by a combination of social integration and neighborhood factors is as follows: total sample, 5.9; all whites, 10.5; all Negroes, 8.1.

must be viewed as "tentative" because of the small number of whites in Ghetto.) The high level of anomie among whites residing in Ghetto, as compared to that of whites in other areas, may possibly be linked to feelings of isolation, based on the reality that whites make up less than 5 percent of Ghetto's population. Among the SES variables, both education and home ownership were negatively correlated with anomie, but occupation showed only a weak and inconsistent association with anomie.

Residents of all three neighborhoods—both Negro and white—who had lived in an area for ten or more years expressed lower anomie than their newer neighbors. Among those who had lived in an area for an intermediate period (from two to ten years), however, no clearcut relationship between length of residence and anomie scores emerged. Long-term residence in a neighborhood thus appeared to be mainly a concomitant of social integration, manifest through low anomie scores.

In Ghetto, anomie was correlated positively with neighborliness but negatively with social participation. Conversely, among Negroes in the other neighborhoods (Northeast and West Side) and among whites in the combined areas, the reverse of this pattern was found: anomie was associated positively with social participation but negatively (or nonsignificantly) with neighborliness. This pattern suggests that the "appropriate" mode of social integration within a neighborhood may be a function of the invasion and succession cycle. In the racially stabilized Ghetto, the favored medium of interaction appeared to be the voluntary organization, while in transitional areas neighboring may have served as the principal integrative process until a more formalized neighborhood organization developed. In general, persons who were out of phase with the dominant interactive mode of their neighborhood expressed higher levels of anomie.

In conclusion, the results of this research indicate that a high level of anomie is not a universal characteristic of the urban Negro ghetto. Also, the usual stereotype of the ghetto as a disorganized area is somewhat inaccurate. The ghetto which was studied in this research had an organized social structure identifiable through standard survey research methods.

In previous studies of anomie, two somewhat divergent trends were reported. Some researchers approach anomie through the traditional "means-ends paradigm" (Merton 1963, pp. 161–94). Conceptualizing anomie principally as the result of blocked access to success goals, the reasoning typically has been to show that anomic persons are those of relatively low status who have been frustrated in their attempts to reach certain goals (Meier and Bell 1959, p. 201; Mizruchi 1964, p. 118).

The second approach can be traced directly to Srole, who conceived of his anomie scale as a general indicator of social integration or a measure of "self-to-other distance" (p. 711). Undoubtedly, the strongest statement

of this position is to be found in Lander's conclusion (1954, p. 89) that anomie is primarily a function of community stability, independent of poverty.

Although the results of the present research give general support to this latter "social integration" explanation, they also lead to a modification of the social integration concept. While most of the previous research suggests that social participation and neighborliness are universal concomitants of social integration, the present study demonstrates that among Negro residents of certain areas, high levels of social participation or neighborliness may be associated with low levels of social integration. Thus, the specific form and content of social integration appear to depend upon the social organization of the individual neighborhood. The impact of differing patterns of neighborhood organization is demonstrated most vividly in the multiple regressions, which reveal that neighborhood and social integration factors, operating jointly, account for as much of the variance in anomie as do the standard measures of individual socioeconomic status and other measures of the social characteristics of individuals.

Although it has been shown that the anomie scores of inner-city residents may be most accurately predicted if the social interaction patterns of separate neighborhoods are analyzed, it has not been explained what kind of social integration process insulates Ghetto from various strains toward anomie, including poverty and a heavy concentration of other social pathologies. In short, why does the standard "relative deprivation" explanation not seem to apply to anomie in Ghetto, an area where individual and collective goals surely can be said regularly to be frustrated?

If a ghetto is sufficiently isolated to maintain its own subcultural norms, the usual attribution of anomie to a feeling of "relative deprivation" would then no longer apply. Ghetto dwellers would seek success not primarily in middle-class terms, but instead in ways in keeping with the shared symbols of a black, lower-class reference group. Moreover, if the ghetto possesses a relatively stable social system, and if this system is kept in balance by its very physical and social isolation—if indeed the area is a "ghetto" in the classical sense—then the subcultural norms of a ghetto should be strengthened constantly by interaction within a homogeneous black, lower-class group. That is, if a ghetto is a highly organized, isolated, subcultural entity, low anomie would result.[25] The present research has presented some evidence that the ghetto in question is organized and somewhat less proof that it is isolated, but whether it is a true subculture is beyond the scope of this inquiry.

[25] That some urban neighborhoods have this self-contained quality has been noted by Gans in his study of one of Boston's Italian neighborhoods which functioned as a community in miniature, as an "urban village," in which the internal neighboring network operated as a closed system for area residents (Gans 1962, pp 3–17).

Wirth was indeed accurate, then, when he stressed the systematic interaction between personal attributes and neighborhood factors in explaining local differences in urban styles of life. That this interaction is complex and elusive is borne out in the finding that anomie in Ghetto is lower than in other adjacent lower-class urban neighborhoods—a finding that runs counter to the sociological convention Particularly as we face our responsibility of coping with the problems of race and the city, it is our hope to spark the sociological imagination in seeking knowledge to solve these problems.

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School Ethnic Composition, Social Contexts, and Educational Plans of Mexican-American and Anglo High School Students[1]

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College plans of Mexican-American and Anglo high school students are related by a model to the following variables: family socioeconomic status, school ethnic composition, measured intelligence, parents' aspirations, and peers' aspirations Data have been obtained from seniors in five urban secondary schools, and path analyses are developed separately for Mexican-American and Anglo boys and girls. Family socioeconomic status is shown to be a weak direct predictor of college plans for all groups but serves as an indirect predictor for Anglos. Both ethnic groups are found to be more apt to develop college plans in schools dominated by their own ethnic group. The other variables also directly affect college plans: peers' aspirations is the most predictive variable for Mexican-American boys, and parents' aspirations the most predictive for the other three groups

INTRODUCTION

The low level of educational achievement among Mexican-American youth in American society represents a continuing social and political problem. In the last few years, this ethnic minority has accelerated its social and political mobilization in an effort to bring about change. This process includes the development of mass militancy, the development of a "brown power" ideology, and pride in *la raza*. Similarities to the early stages of the Black Power movement are obvious. It is anticipated that, as with blacks, the explosion of expectations of Mexican-Americans will generate considerable social research.

In the last three years, the community of 800,000 Los Angeles Mexican-Americans has become highly involved with the structure of educational institutions and is pressing demands for equality of educational opportunity.[2] A major focus of this new militancy is the city's secondary educa-

[1] This study was conducted at the Center for the Evaluation of Instructional Programs, Graduate School of Education, University of California, Los Angeles, under a grant from the Ford Foundation. We wish to acknowledge the help of John Stern and Lillie Durr of UCLA in the data analysis Many helpful comments and suggestions were provided by Dr. William G Spady of the Ontario Institute for Studies in Education and by Professor Richard P. Boyle of UCLA.

[2] Among the more important of the thirty-eight demands on the school district com-

tional system. The inveterate dissatisfaction of students and parents with this system has led to a series of class boycotts and demonstrations. This constitutes the first act of mass militancy by urban Mexican-Americans in Southern California.[3]

There is a paucity of information on the education of Mexican-American youth, but recent research in this area makes it possible to describe the magnitude of the problem. A recent national survey by Coleman et al. (1966), *Equality of Educational Opportunity*,[4] indicates that for a number of achievement tests the ranking from high to low performance is Anglos, Orientals, American Indians, Mexican-Americans, Puerto Ricans, and blacks. About 85 percent of the Mexican-American pupils in the Coleman sample are below the Anglo average.

Such differences between Anglo and Mexican-American groups are greater in the higher age groups, indicating that the gap between the two groups probably increases over time. In verbal ability, for example, Mexican-Americans lag two years behind the Anglo level in grade 6, but 3.5 years in grade 12. This large discrepancy underestimates the aggregate differences for age groups of Mexican-American and Anglo youth who begin the educational process at the same time. Important to the aggregate differences are the higher "drop-out" rate among Mexican-Americans, especially by the least "capable" students, and the reduction of scholastic educational performance consequent to withdrawal. Thus, comparisons of Mexican-American and Anglo youth *in* school underestimate differences for entire age groups.

This lack of educational attainment is perceived by Mexican-Americans as a barrier to social mobility. Grebler (1967, p. 1) reports that Southwest Mexican-Americans cite lack of education as the greatest barrier to group improvement, with matters such as labor-market discrimination, lack of participation in government, and lack of ethnic-group organization as secondary though important factors.[5]

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pulsory bilingual and bicultural education, reduction in class size, a higher counselor-student ratio, and an end to corporal punishment (see McCurdy 1968)

[3] Ralph Guzman states that the movement has radicalized the Mexican-American community in opposition to the Anglo-dominated power structure and predicts that violent confrontation is a possible outcome (see Torgerson 1968)

[4] The Los Angeles School District did not participate in this study

[5] As the Mexican-American community develops militant ideology, educational opportunity may come to be perceived as a less central barrier than more general patterns of institutional and interpersonal discrimination This change in emphasis has occurred in the black community since *Brown* v *Board of Education* in 1954. Carmichael and Hamilton (1967, pp 19–20), e g, in emphasizing that equality of educational opportunity is no panacea for the black community, point out that unemployment rates in 1965 were higher for nonwhite high school graduates than for white high school dropouts. They cite Brimmer (1966, p. 260) to the effect that a nonwhite man must

Demographic data indicate that this concern with the amount of education is not misplaced. In 1960, in the Southwest, Mexican-Americans had a median of 7.1 years of schooling, compared with 9 0 for nonwhites (blacks, Orientals, . . .) and 12.1 for Anglos. The Mexican-American median varies from 8.6 in California to 4.8 in Texas, compared with Anglo figures of 12.2 in California and 10.3 in Texas (Grebler 1967). The "schooling gap" is least in California. Yet discontent with the educational system is perhaps the most intense in this state. This is not paradoxical, as minorities characteristically experience increasing frustration and "psychological losses" as their position vis-à-vis the majority shows actual gains. Such a phenomenon has been shown to result from the fact that aspirations increase more rapidly than do attainments (Pettigrew 1962). And Mexican-Americans have increased their relative position: the overall educational attainment of Spanish surname persons twenty-five years of age has increased from 5.4 to 7.1 years of schooling in the period from 1950 to 1960, a greater gain in years than experienced by Anglos in that same time (Grebler 1967; Gonzales 1965; Simpson and Yinger 1953; Singleton and Bullock 1963).

Overall gains in years of education for Mexican-Americans are reflected in generational comparisons. Southwest Mexican-Americans of age fourteen have more schooling than do Mexican-American adults: in 1960, this group had 9 2 years of schooling as compared with 7.1 for adults over twenty-five years of age. For Anglos, the obverse is the case, adults having more years of schooling than do fourteen-year-olds. It should be noted that years of schooling is a most superficial yardstick of educational attainment (Grebler 1967, pp. 1–15), and discontent with education focuses not on the quantity of secondary schooling but upon its quality.

The data for this study are derived from a larger cross-sectional investigation by Gordon et al. (1968) carried out in a number of public schools in the Los Angeles School District. The present paper is a comparative analysis of processes by which Mexican-American and Anglo high school students develop concrete plans to go to college. The data are analyzed by means of a model of educational behavior, and each of four ethnic-sex groups are analyzed separately. The purpose of this procedure is to: (1) provide data on a number of structural and interpersonal variables on the college plans of high school students, (2) arrange these variables in a

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have between one and three years of college before he can expect to earn as much as a white man with less than eight years of education

Mexican-Americans in the United States have increased their relative educational position vis-à-vis Anglos. But they have *not* improved in occupational mobility Of all ethnic groups in the United States, Mexican-Americans are the only group whose members show no intergenerational increase in socioeconomic circumstances (Bogue 1959, p. 372). Thus, Mexican-Americans do share with blacks the problem of converting education into social and economic status.

plausible model, (3) compare the direct and indirect effects of variables within and between the ethnic-sex groups. Comparisons are made between Mexican-American and Anglo students, and differences within and between the two ethnic groups are discussed.

VARIABLES AFFECTING EDUCATIONAL PLANS

In recent years, an increasingly complex view of processes by which adolescents are led to the college-going decision and subsequent educational attainment has emerged. Family socioeconomic status (SES) is, almost without exception, found to directly affect college plans. Children of high social class origins are more apt to aspire to go to college, are more apt to make concrete plans to do so,[6] and are more apt to actualize these plans. This effect persists when related variables such as sex, measured intelligence, and neighborhood status are controlled (Sewell and Shah 1968, p. 560).

Sewell and Shah (1968) show that parental encouragement of college going is an intervening variable between family SES and college plans. That is, family status indirectly affects college plans as follows: children from high social class backgrounds are more apt to have parents who encourage and even expect their children to go to college. Parental stress on college, in turn, makes it more probable that children will go to college, since children respond to such parental aspirations (Kahl 1953; Bordua 1960).

Measured intelligence (IQ) is also found to be a factor in formulation of college plans. The position of this variable in a causal model is subject to varying interpretations. Turner (1964, pp. 49, 52), in a study of adolescent aspirations, treats it as in part determined by social class. That performance on intelligence tests is influenced by social class and other environmental factors is no longer controversial (Klineberg 1935). What *is* questionable is whether such tests lead to valid inferences about *innate* intellectual capabilities of children from varying socioeconomic and ethnic backgrounds (Jensen 1969; Deutsch 1969; Boyer and Walsh 1968; Rosenthal and Jacobson 1968). In this paper, measured intelligence is interpreted as a measure of *general academic capability*. That is, if students learn to do well on such general written tests, then they have acquired the capability to do well on examinations and hence to be defined as good students. In the model, it is posited that this variable is, in part, determined by the ethnic and social environments of the school and family.

The perception of oneself as "intelligent" should make it more probable that plans to go to college will be developed. This *direct* predictive value

[6] For references to these studies, see, for openers, Sewell, Haller, and Strauss (1957), and Sewell and Armer (1966)

of measured intelligence is reported by Rossi (1961, pp. 269–73), who claims that IQ explains 40–60 percent of the variance in student performance and that holding IQ constant reduces the association between performance and other predictor variables.

Measured intelligence might also have *indirect* effects via parental aspirations and the aspirations of peers. If parents perceive a child as being intelligent, they are more apt to encourage him to seek higher education; the child is in turn more likely to make definite plans. If the child belongs to a high school peer group whose members aspire to higher education, the value climate of the group reinforces making college plans, making it more likely that the child will develop these plans.

The ethnic composition of a school's student population is also expected to influence college plans. A low-status ethnic group which comprises a numerical minority in a school typically is ignored or excluded by Anglo students or may be equally abused by teachers. If an ethnic group is a numerical majority, Anglo youth in the school are apt to band together or to be oriented to peers outside of school; whether numerically a majority or a minority, Anglo youth are not necessarily affected in their plans by the presence of minorities. These processes have been found to operate for black and white students in an urban area (Gottlieb and TenHouten 1965).

An ethnic minority, however, is less able to afford to ignore and not interact with the dominant Anglos in a school system. Teachers will be predominantly Anglo, even if the student body is not: faculty integration almost always lags behind student-body integration. Further, the economic values and occupational roles students are prepared to fill are those of Anglo-dominated society. For this reason, it can be expected that the ethnic-density variable should be a more effective predictor of college plans for Mexican-Americans than for Anglos.

Anglo teachers in the sample schools tend to be viewed as unable to understand the problems of Mexican-American students and are seen by these students as obstacles to the attainment of the appurtenances of adulthood. There is a resultant "craving for emancipation" from teachers and the school system.[7] This phenomenon is, to some extent, shared by all adolescent groups, but it is particularly intense in groups ascribed low status in society. For Mexican-Americans, it is intensified by cultural definitions. Given the above orientation to the school system, it is hardly surprising that Mexican-American students would develop "opposing ideals" (Blau 1964, pp. 224–52, 288–89) vis-à-vis the formal school system in predominantly Anglo schools.

Confronted by a higher-status group possessing greater access to college

[7] On the traditional noninvolvement of Mexican-American students with their teachers, see Heller (1966, pp 47, 50), and Sherif and Sherif (1964)

education and which places positive value on this outcome, the Mexican-American students are apt to respond by rejecting these values. If there are fewer Anglos, the rewards are less dominated by Anglo students, and the propensity to respond by rejecting achievement values is lower. Consequently, opposition to the school system and its definitions of successful performances, for the Mexican-American students, should be greater in the predominantly Anglo schools, so that low ethnic density should depress college plans for Mexican-American students.

Anglo youth, on the other hand, have ready access to the formal rewards of their schools, regardless of ethnic compositions. Thus, they should not develop opposing ideals when they are a numerical minority. They should likewise be more apt to formulate college plans in the predominantly Anglo schools, where value climates generally place greater rewards on college going and educational attainment.[8]

The foregoing discussion can be partially summarized by a "path diagram" (see fig. 1) relating the five independent variables to college plans. In this figure, the curved line represents the correlation among the independent variables not hypothesized to be causally related. The directed straight lines represent the possible path from variable *j* to variable *i*. The method of path coefficients is employed, so that for each directed line there

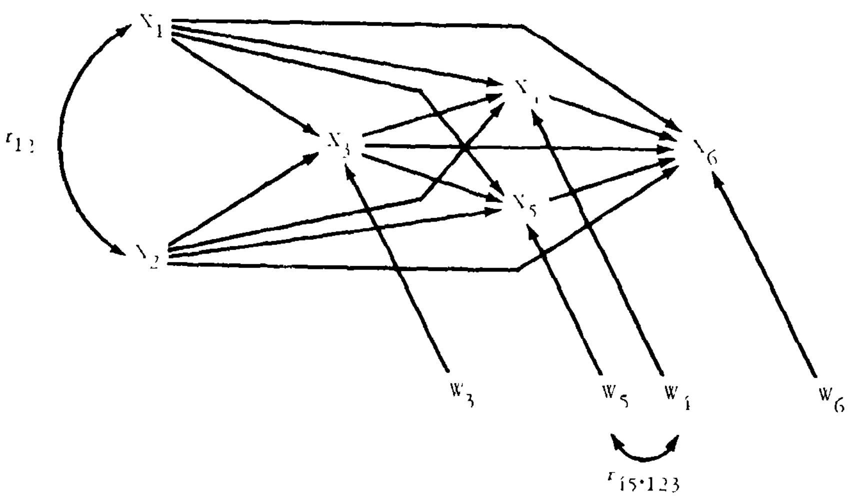


FIG 1—Model for the path analysis relating five independent variables to college plans. X_1, family socioeconomic status; X_2, ethnic density (percentage Mexican-American), X_3, measured intelligence; X_4, parents' aspirations; X_5, peers' aspirations, X_6, college plans

[8] In coding this variable, professional, technical, and kindred workers = 7, . . . , laborers = 1. If the student's father is deceased or the home is female headed, the mother's occupation is used to represent the family If data are missing for one indicator, the mean score for the student's ethnicity and school are used.

is a path coefficient p_{ij} measuring the effect of variable j on variable i, independent of the effects of all other variables leading into variable i.

The model shown by the path diagram is subject to a basic limitation: it does not include all the substantive variables known to be relevant to educational plans. Second, path analysis requires the assumption of a well-defined social process This in itself is good, but it should be kept in mind that alternative models can be constructed for the same set of variables Third, path diagrams tested with cross-sectioned data cannot *prove* causality. However, path analysis does provide a means for analyzing the strength of direct and indirect effects within an ordered set of variables

THE SAMPLE

The secondary data presented here are obtained from the study of five public high schools. The district's fifty-five schools were first stratified by the percentage of Mexican-American students (ethnic density), estimated by the percentage of students with Spanish surnames from 1960 Census data. Schools with less than 10 percent Mexican-American students were excluded, since the resulting samples would be too small for ethnic comparisons. A number of atypical schools having special programs were also excluded. It was possible to gain entry into five schools The selected schools are generally representative of the district's schools, but inferences are not made to this population of schools

To obtain a sample of the desired number of students, a sample of 68 percent of the twelfth-grade morning social studies classes were chosen at random. A nearly random sample of students was obtained, as these classes are taken by all seniors, and assignments to morning or afternoon classes were made randomly. A small bias is introduced by variability in class size, so that students in smaller classes were slightly overchosen.

Parental consent for interviews and access to cumulative records was obtained for 60 percent of the students, the attrition being about the same for both ethnic groups. The self-reported grades of participating students differed very little from the official grades of seniors in the high schools not sampled. In addition, the general socioeconomic statuses of participating students were slightly higher than the statuses of those not participating.

The initial sampling procedure yielded 1,404 students from the original population of 3,489. The exclusion of nonwhite minorities (100 blacks, 122 Orientals, forty-two other nonwhite) was necessitated by the small sizes of these groups. The final sample consisted of 315 Mexican-American boys, 309 Mexican-American girls, 226 Anglo boys, and 229 Anglo girls.

Data from the students were obtained via a questionnaire. Because Mexican-American students do not read English as well as do Anglo students, the questionnaires were read to everyone.

The dependent variable for the study is college plans. The independent variables are family SES, school ethnic density, measured intelligence, parents' aspirations for the student's college plans, and the aspirations of the student's peers to go to college.

The empirical indicator for the variable *family SES* (X_1) is based on the father's occupational status and the educational level of both parents. The seven major socioeconomic groupings of occupations used by the U.S. Census Bureau provide a crude measure of the father's occupational status.[9] The mean years of schooling of both parents is used as the second dimension of family SES. The final index used for this analysis is a mean of (*a*) the father's occupational status score, and (*b*) the mean years of schooling of both parents. It is deemed necessary to include both indicators since many Mexican-Americans are well-educated and are "middle class" in their life styles, but are relegated to low-status manual jobs. For this reason, excluding education from a family SES measure or including it through socioeconomic scores for detailed occupations leads to underestimation of family SES for Mexican-Americans.

The variable *ethnic density* (X_2) is defined as the percentage of Mexican-American students enrolled in the selected school. The five schools have percentages 65, 55, 21, 20, and 11.

The Henmon-Nelson Test of Mental Ability (Henmon and Nelson 1942) is used as an indicator of *measured intelligence* (X_3). For the number of students for whom scores are not available, the mean Henmon-Nelson score for the school's ethnicity and sex is used.

The variable *parents' aspirations* (X_4) is measured by the following question regarding the student's perceptions of his parents' educational aspirations for his or her educational career: "My parents would like me to: (1) quit school as soon as possible; (2) graduate from high school only; (3) go to a trade school; (4) go to junior college; (5) go to a four-year college; (6) go to graduate school after college."

The variable *peers' aspirations* (X_5) measures the student's perceptions of his best friends' aspirations for their own educational future. Students are asked: "Think now of your best friends. How many of them want more

[9] The ethnic density of the schools was negatively correlated with school SES measured by the mean family SES score of the students ($r = -.84$). Thus, ethnic density is an inverse measure of school SES. Since school SES is sometimes used as an indicator of a school's "value climate," the high-ethnic-density schools may have value climates which place less emphasis on college going and achievement. This application of school SES has been used by Wilson (1959), Ramsøy (1961), Michael (1961), and Turner (1964) A study of twenty high schools carried out by McDill, Meyers, and Rigsby (1967), however, indicates that value climates in which intellectualism, achievement, and competition are stressed may occur in either high- or low-SES schools and that a school's SES level may not be an adequate measure of normative climate. In this study, this implies ethnic density may not be an adequate index of normative value climate

education after high school? (1) None of them; (2) some of them; (3) most of them; (4) all of them."

The dependent variable *college plans* (X_6) is a measure of the student's aspiration level for himself as indicated by responses to the alternatives offered for the question on parents' aspirations.

Variables X_4 and X_6 are scored 1 through 6 as shown above: variable X_5, 1 through 4 as shown above. The assumption of interval level measurement is not actually met, but is employed for parametric statistical analysis. The analysis described below is found to be robust for this assumption, since the same analysis with ordinal variables dichotomized provided essentially the same results.[10]

THE DATA

Table 1 shows the scores for the two background variables. Mean family SES has a weak negative correlation with ethnic density ($r = — 11$). In all five schools, the mean family SES scores are higher for Anglos than

TABLE 1

CHARACTERISTICS OF SAMPLED SCHOOLS PERCENTAGE MEXICAN-AMERICAN AND MEAN FAMILY SOCIOECONOMIC STATUS

School	Mexican-American (%)	Mean Family SES		
		Mexican-American	Anglo	Total
A	65	3 2	5 2	3 4
B	55	3 3	3 7	3 4
C	21	3 0	4 2	3 7
D	20	2 8	4 0	3 6
E	11	4 1	5 4	5 2

for Mexican-Americans. There are variations in the six variables by ethnicity. The scores for each variable, in table 2, show that the Anglo youth we are considering come from families with relatively high SES, attend schools primarily with other Anglos, and score higher on the Henmon-Nelson test. The variables parents' aspirations, peers' aspirations, and college plans also show ethnic differences. Anglos are more apt to perceive that their parents want them to go to college, that members of their peer groups aspire to go to college, and to report they have definite college plans.

There are also differences in the standard deviations for three variables. The Anglo students come from families with more varied socioeconomic

[10] Evidence for the robustness of path analysis with respect to the assumption of interval level measurement in the variables, and the resulting justification for this assumption for ordinal data, is provided by Boyle (1970)

TABLE 2

MEAN SCORES AND STANDARD DEVIATIONS FOR EACH VARIABLE, BY ETHNICITY AND SEX

	MEXICAN-AMERICANS		ANGLOS	
	Boys	Girls	Boys	Girls
Variable mean scores:				
Family SES	3 1	3 0	4.3	4 4
Ethnic density	46 8	46 1	19 2	18 2
Measured intelligence	91 0	90.0	104 0	104 0
Parents' aspirations	4 1	3 8	4 7	4 4
Peers' aspirations	2 8	2.3	3.1	3 0
College plans	3 5	3 2	4 2	4.0
Variable standard deviations				
Family SES	0 89	0 86	1 11	1 14
Ethnic density	18 82	20 18	9 80	8 32
Measured intelligence	9.44	8 96	13 80	12 66
Parents' aspirations	1 18	1 36	1 13	1 22
Peers' aspirations	0 71	0.73	0 77	0 77
College plans	1 16	1.25	1.30	1 30

levels, and have more varied IQ scores. For these variables, the Mexican-American's scores are skewed to the right, since they are less apt to have high SES families or obtain high IQ scores. For ethnic density, the Mexican-American values have much larger deviations, since they are more apt to attend predominantly Anglo schools than vice versa, that is, they are more "integrated" ethnically.

The statistical analysis carried out can be summarized as follows. Gross relationships of the independent variables to college plans are measured by zero-order correlation coefficients. Multiple regression analyses are carried out on the standardized scores for the variables. These analyses are then interpreted by the method of path coefficients.

RESULTS

The zero-order correlation matrices are shown in table 3. For three of the four groups, parents' aspirations is more highly correlated with college plans than is any other variable: for Mexican-American boys, it is the second strongest variable. Peers' aspirations also has a strong relationship to college plans in all four groups: for Mexican-American boys, it is the strongest predictor of college plans. Family SES and measured intelligence are stronger predictors of college plans in the Anglo groups than in the Mexican-American groups. In part, this may result from the lower standard deviations of these variables in the Mexican-American groups (table 3). Ethnic density is positively correlated with college plans for Mexican-American students but is negatively correlated with college plans for Anglo students.

TABLE 3

ZERO-ORDER CORRELATION MATRICES, BY ETHNICITY AND SEX

Variable	X_1	X_2	X_3	X_4	X_5	X_6
Mexican-American boys:						
X_1 (family SES)		04	16	15	12	15
X_2 (ethnic density)			−13	02	12	10
X_3 (measured intelligence)				29	05	13
X_4 (parents' aspiration)					19	31
X_5 (peers' aspiration)						33
X_6 (college plans)						
Mexican-American girls						
X_1 (family SES)		03	05	05	−02	11
X_2 (ethnic density)			−05	10	16	17
X_3 (measured intelligence)				24	09	18
X_4 (parents' aspiration)					17	36
X_5 (peers' aspiration)						23
X_6 (college plans)						
Anglo boys.						
X_1 (family SES)		−31	24	24	18	26
X_2 (ethnic density)			−15	−02	−12	−08
X_3 (measured intelligence)				39	26	41
X_4 (parents' aspiration)					14	50
X_5 (peers' aspiration)						32
X_6 (college plans)						
Anglo girls.						
X_1 (family SES)		−32	33	32	29	38
X_2 (ethnic density)			−18	−16	−12	−23
X_3 (measured intelligence)				30	23	38
X_4 (parents' aspiration)					26	60
X_5 (peers' aspiration)						42
X_6 (college plans)						

The data for the model given in figure 1 are shown in table 4. The direct and one-way indirect effects of causally antecedent independent variables in the model are shown in table 5. The results of the path analyses will be discussed separately for each independent variable

Family SES

The data in table 5 show this variable is a relatively weak *direct* predictor of college plans in all four groups. For the Mexican-American students, the direct effects are boys .07 and girls .09; for Anglos, boys .09 and girls .10 These values do not differ significantly by either ethnicity or sex. There are, however, ethnic differences in the *indirect* effects of family SES. For Mexican-Americans, the indirect effects are weak and insignificant: they add up to .08 for boys and .02 for girls. For Anglos, the indirect effects are consistently weak and positive: for boys the indirect effects add up to .17; for girls, to .24.

Thus family SES has a much stronger relationship to college plans for

TABLE 4

PATH COEFFICIENTS, RESIDUAL EFFECTS, AND MULTIPLE CORRELATION COEFFICIENTS, BY ETHNICITY AND SEX

	Paths into X_3		Paths into X_4			Paths into X_5			Paths into X_6				
	p_{31}	p_{32}	p_{41}	p_{42}	p_{43}	p_{51}	p_{52}	p_{53}	p_{61}	p_{62}	p_{63}	p_{64}	p_{65}
Mexican-American													
Boys	17	—.14	11	.05	28	.10	12	05	07	06	.05	24	.26
Girls ...	05	— 05	02	11	.25	—.03	17	10	09	.11	08	.30	.16
Anglo:													
Boys . .	21	— 08	.18	.09	.36	.12	— 05	22	09	01	.19	.38	.20
Girls ...	30	— 09	24	— 04	21	23	— 02	.16	10	— 07	14	45	23

	Residuals					Multiple Correlations			
	W_3	W_4	W_5	W_6	$r_{45 \cdot 123}$	$R_{3 \cdot 12}$	$R_{4 \cdot 123}$	$R_{5 \cdot 123}$	$R_{6 \cdot 12345}$
Mexican-American.									
Boys . .	97	95	.98	90	.17	22	31	17	43
Girls ...	99	.96	98	90	13	07	.27	19	43
Anglo:									
Boys .. .	97	.90	96	80	03	25	43	29	60
Girls ..	94	92	95	72	16	34	38	32	69

TABLE 5

PATH ANALYSES. DIRECT AND INDIRECT EFFECTS OF FIVE VARIABLES ON COLLEGE PLANS, BY ETHNICITY AND SEX

	Direct Effects	One-Way Indirect Effects				
		Via X_3	Via X_3X_4	Via X_3X_5	Via X_4	Via X_5
Mexican-American boys.						
X_1 (family SES)	.07	.01	01	00	.03	03
X_2 (ethnic density)	07	— 01	—.01	00	01	03
X_3 (measured intelligence) . .	05	.	.	.	.07	01
X_4 (parents' aspirations)	24	..	..	.	...	.
X_5 (peers' aspirations)	26	..	..	..	...	.
Mexican-American girls.						
X_1 (family SES)	09	01	00	00	.01	00
X_2 (ethnic density)	11	— 01	00	00	03	03
X_3 (measured intelligence)	.08	.	..	...	08	02
X_4 (parents' aspirations)	.30	. .	...	...	.	
X_5 (peers' aspirations)	16	. .	..	...	...	. .
Anglo boys						
X_1 (family SES)	09	04	.03	.01	07	.02
X_2 (ethnic density)	01	— 02	— 01	.00	.03	— 01
X_3 (measured intelligence)	.19	.	. .	. .	.14	04
X_4 (parents' aspirations)	38	...	...	...	...	..
X_5 (peers' aspirations)	.20	...	...	...	...	...
Anglo girls:						
X_1 (family SES)	10	04	03	01	.11	.05
X_2 (ethnic density)	—.07	— 01	—.01	.00	—.02	.00
X_3 (measured intelligence)	15	...	...	...	.09	04
X_4 (parents' aspirations)	.45	. .	...	...	..	...
X_5 (peers' aspirations)	23	...	...	...	...	...

the Anglo groups ($r = .26$ for boys and .38 for girls) than Mexican-American groups ($r = .15$ for boys and .11 for girls), although the net effects are low and about equal for all groups. Ethnic differences are found in the indirect effects, because family SES is substantially associated with other intervening variables for Anglos in the model. Part of the indirect effects involves measured intelligence, but those most noticeable are via parents' aspirations: for Anglos, these effects are boys .07 and girls .11. Anglo youth in a middle-class family environment are, in general, more apt to be encouraged by their parents to seek higher education, and this help is effective. Sewell and Shah (1968) also observe these direct and indirect effects in a large sample of high school students. Their sample data show stronger effects for family SES than do the data given here.

If any tentative generalization can be advanced for an ethnic comparison, it is that social class is more important to achievement for Anglos than for Mexican-Americans. This may in part be due to the fact that both groups are ascribed status in society by family class level, but Mexican-Americans are ascribed low status by their ethnicity as well. Mexican-Americans may be middle-class, but they are still, after all, Mexican-Americans. A second factor might be the limited relevance of the *kind* of education that parents of Mexican-American youth have. The data (Gordon et al. 1968) show parents' education to be a poor predictor of college plans. It has also been suggested that the quality of high school education may be lower for Mexican-Americans than for Anglos. A third factor in the ethnic difference may result from the fact that family SES is less variable in the Mexican-American groups, so lower predictive power for the variable may be a statistical artifact.

Ethnic Density

There is, as anticipated, a directional difference in the effects of ethnic density on the college plans of Mexican-American and Anglo students. For Mexican-Americans, this variable is positively related to college plans: the direct effects are boys .07 and girls .11. For Anglos, ethnic density is unrelated to college plans for boys and is negatively related to college plans for girls: the direct effects are boys .01 and girls —.07. Thus, school composition has low effects among boys but produces an interaction among girls: Mexican-American girls are to some extent "overwhelmed" in mainly Anglo schools, while Anglo girls may be "held back" in ethnically mixed schools.

For Mexican-Americans, the direct effects of ethnic density are larger than the total indirect effects (zero-order correlations minus direct effects), while the obverse is true for Anglos. The total direct and indirect effects are Mexican-American boys .07 and .03, Mexican-American girls .11 and

.06, Anglo boys .01 and —.09, and Anglo girls —.07 and —.16. It should be noted that ethnic density has a low direct effect in all four groups. Both Mexican-American and Anglo students are more apt to have college plans if their own group is numerically dominant: in this sense the two groups may serve as negative reference groups for each other.

Ethnic density contributes little to the explanation of college plans that is not accounted for by the other variables in the model. It is, in general, the weakest predictor variable in the analysis.

It was expected that the effects of ethnic density would be absolutely greater for Mexican-Americans than for Anglos. This result is also obtained, although the differences for direct effects are not significant.

The data show that Anglo youth do not increase their propensity to develop college plans when a high proportion of students are Mexican-American. Anglos apparently do not profit from Mexican-Americans who have lower status in the society and the school and who, consequently, do not function as reference groups and role models. Also, the cross-ethnic rates of interaction are low: informal social systems of high school ethnic groups are relatively distinct.[11] In addition to not gaining from the Mexican-American students, the presence of Mexican-American students means there are fewer Anglo students present who can function as part of a reference group.

Measured Intelligence

High family SES contributes to high IQ test scores, since the data in table 3 show these two variables to be positively correlated in all four groups. Some variation in IQ is explained by ethnic density, since the variables are negatively correlated in all four groups. Measured intelligence is, in turn, correlated positively with parents' aspirations, peers' aspirations, and college plans in all four groups.

In the model represented by figure 1, it is possible for measured intelligence to influence college plans in three distinct ways. First, IQ might function as an intervening variable between either of the two structural variables and college plans. Table 5 shows the indirect effects of these two variables via the three intervening "personal" variables, IQ, parents' aspirations, and peers' aspirations. There are six such possible effects for each group. Such effects are substantively plausible but are observed empirically only for family SES among Anglos. There are weak effects via IQ (boys

[11] See, e g , Jessor et al (1968, pp 272–73). In this study of a small tri-ethnic community (Anglo, Mexican-American, American Indian) it is found that students choose to or actually do spend time with their own ethnic group and are exposed to informal controls primarily from members of their own group A high rate of cleavage in informal Mexican-American and Anglo groups is also observed by Sherif and Sherif (1964).

.04, girls .04) and via IQ and parents' aspirations (boys .03, girls .03). Although in part caused by background factors, IQ is not shown by these data to function as an intervening variable between the background factors and college plans.

Measured intelligence *does* influence college plans in the other two ways, as an independent variable. First, it has direct independent effects on college plans: Mexican-American boys .05, Mexican-American girls .08, Anglo boys .19, and Anglo girls .15. The variable has stronger effects for Anglos than for Mexican-Americans.

Second, IQ contributes to college plans with parents' aspirations and, to a lesser extent, peers' aspirations, serving as intervening variables. These effects of IQ via parents' and peers' aspirations are Mexican-American boys .07 and .01, Mexican-American girls 08 and .02, Anglo boys .14 and .04, and Anglo girls .09 and .04. For each group, the effect via parents is greater than the effect via peers. No consistent influences on the control variables ethnicity and sex are observed.

The two variables family SES and IQ have been found to be the two most powerful predictors of academic performance (see Boocock 1960, and Lavin 1965, for reviews of these studies). But, for the present study, this is not the case, since parents' and peers' aspirations are stronger variables for all groups, and ethnic density has stronger direct effects for the Mexican-American students.

It is easy to place too much substantive emphasis on the differential effect of IQ on the college plans of Mexican-American and Anglos, since the variable is restricted and skewed among the Mexican-Americans.

Parents' Aspirations

Sewell and Shah (1968) find the direct effect of parents' aspirations to be stronger than either family SES or IQ for both boys and girls. This finding is upheld here: for all four groups, this variable has a stronger positive effect

Parental aspirations have a lesser effect on college plans for Mexican-Americans than for Anglos. The effects are Mexican-American boys 24, Mexican-American girls .30, Anglo boys .38, and Anglo girls .45 It may be that the lower overall relationship of parental experience, information, and cultural background among Mexican-Americans inhibits the effectiveness of their encouraging their children to obtain college educations. Likewise, as parental aspirations are reported as perceived by the child, it may be that Anglos are more likely to describe consistency in parents' aspirations and their own plans.

Parental aspirations are more predictive of college plans for girls than for boys among both Anglos and Mexican-Americans. Possibly, boys re-

spond to other sources of encouragement and to societal incentives which are not as strong for girls. There are thus direct positive influences of the control variables on the effectiveness of parental aspirations for Anglo status and female status.

Peers' Aspirations

Finally, we consider the peer environment of the school. The variable peers' aspirations shows a significantly positive direct effect in all groups. The values of the path coefficients are Mexican-American boys .26, Mexican-American girls .16, Anglo boys .20, and Anglo girls .23. While these values do not differ widely by ethnicity and sex, the importance of peers' aspirations relative to the other independent variables is greater among the Mexican-American students. For Mexican-American boys, it is the strongest variable, while for the other three groups it is the second strongest variable. These outcomes indicate that socialization in the peer group needs to be considered in any realistic causal model describing processes that lead to college plans and subsequent educational attainment. Further, the most powerful source of aspirations for Mexican-American boys is the aspiration level of their closest friends. This outcome suggests that popular notions regarding antiacademic norms of Mexican-American youth should be critically evaluated.

For three of the groups, the most important determinant of college plans is parents' aspirations. The one exception is the Mexican-American boys, who are as much peer as parent oriented. For the Mexican-American boys, the direct effects of parents and peers are .24 and .26; for the other three groups, the effects of parents are about twice as important: Mexican-American girls .30 and .16, Anglo boys .38 and .20, and Anglo girls .45 and .23.

CONCLUSIONS

Each of the five independent variables appear to be involved in the processes by which students do or do not develop concrete plans to pursue higher education. The analysis suggests that ethnic comparisons are important to research on the educational plans and achievements of youth. There is a lack of data comparing Mexican-American and Anglo youth and still less data dealing with blacks, Mexican-Americans, and Anglos. Such comparative data are not only of intrinsic interest, but specification of such analysis can suggest processes not apparent in ethnically homogeneous or combined groups.

The model used in the analysis is only modestly successful in explaining variance. The coefficients of multiple determination for the effects of the

five variables, $R^2_{6\ 12345}$, are for Mexican-American boys .18 and girls .18, and for the Anglos boys .36 and girls .48. Thus, in each analysis, less than one-half of the variance in college plans is accounted for. More variance is explained for Anglos than for Mexican-Americans. This may reflect differential errors in the equations and/or differential errors in the variables. Since the variable ethnic density is a more effective predictor for Mexican-Americans than for Anglos, it is unlikely that there are more errors in the equations for Mexican-Americans than for Anglos, and it is more likely that the differences derive from the Anglos having higher language skills than do the Mexican-Americans. Despite the fact that the students did not read the questions, verbal skills contribute to answers that are logically consistent and substantively reliable and valid. Thus, apart from real differences in the causal processes between groups, it is easier to explain variance in groups educated to have high verbal skills than in groups with low verbal skills.

A second possible factor in the higher R^2s for Anglos could result from measurement bias in the question on parents' aspirations. Since students report what they believe are their parents' aspirations for them, their own perceptions are reflected in the answers. A need to conform to parental desires may lead Anglos to ascribe aspirations congruent with their parents more than do Mexican-Americans. This is supported by another finding in the Gordon study, where the variable "independence from parental control" is inversely related to college plans among Anglos and directly among Mexican-Americans. Also, the ratios of R^2 values, Mexican-Americans to Anglos, are roughly proportional to the corresponding ratios of direct effects of parents' aspirations on college plans.

The present investigation has been carried out in a single metropolitan area and is based on a relatively small number of students and only five schools. Certainly, both the findings and the processes suggested to account for them require further research. Data need to be gathered from a variety of environments, with more adequate measurement and statistical control. Further, if values and family roles within ethnic groups are related to achievement, definitive research would require comparative analyses of the students' home and school experiences.

The study on which this paper is based was conducted without consulting the Mexican-American community. A certain amount of antagonism to the study and the interpretation of the data thus resulted.[12] This may have produced overrepresentation of politically conservative respondents. It may also be the case that parents giving permission for their children's interviews are not a representative sample of the Mexican-

[12] A highly critical analysis of the Gordon study is provided by Hernández (1970). She examines the value assumptions and survey procedures used in the collection and analysis of data for this study

American population in that militant Mexican-Americans may have been less apt to cooperate with the study.

As a final comment, there is an implicit value judgment involved in defining college plans and academic achievement as desirable outcomes for minority students. The rewards for such attainment are often not forthcoming, due to economic discrimination. At the same time, there are costs involved in educational achievement. These costs could include alienation from, and rejection of, Mexican-American culture. The data for this study show that the Mexican-American students with college plans are more independent from their parents and have lower self-esteem than Mexican-Americans with no college plans. Given these doubtful economic rewards and high costs, it may be rational for Mexican-American youth to decide the price of admission to Anglo-dominated middle-class America is too high.

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Family Status, Socialization, and Student Politics: A Multivariate Analysis[1]

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Instead of bivariate analyses which have been employed in the past to explain the origins of student politics, a multivariate model appears better suited to test the theoretical assumptions available for verification. A sample of 1,246 college students representing both flanks of the campus ideological spectrum has been surveyed from ten universities and from two national conventions. Gamma and path-analysis techniques were used to test the relationships among selected aspects of family status, socialization, and student politics. While both techniques detected family politics to be the strongest predictor of student politics, with offspring closely following the political views of their parents, path analysis revealed that: between 20 and 40 percent of the total variance in the direction of student politics could be explained by the variables in our model; ethnicity was deleted from the model due to its high interrelationship with family politics and parents' religion; both social class and family politics directly affected student politics but had no effect on socialization; and religion alone was able to explain direction of student politics directly and sequentially through family political argumentation.

This paper examines the extent to which the family of orientation, in terms of its status characteristics and socialization patterns, has influenced the origins of campus-based politics that have emerged in the United States during the 1960s. Past research on family status, socialization, and student political activism has operated out of simple, bivariate causal analyses explaining the relationships among: (1) social status and family socialization, (2) family socialization and student politics, and (3) social status and student politics. The prototype theoretical explanation for these studies has been among a series of independent variables as they affect or explain variance in a series of dependent variables. However, if one examines these research designs in more detail, a multidimensional model emerges with at least three sequentially related dimensions: (1) the independent dimen-

1 Revised version of a paper presented at the 1969 annual meetings of the American Sociological Association, San Francisco, California, September 2, 1969 Acknowledgment is extended to David L. Westby and Rex H. Warland, whose helpful suggestions were incorporated into this paper. Robert G. Bernreuter, Vice-President for Student Affairs, and Thomas F. Magner, Assistant Dean for Research of the College of Liberal Arts, Pennsylvania State University, provided funds for the data collection and data processing phases of this project and for the computations which preceded the analysis

sion of family status, (2) the intervening dimension of family socialization, and (3) the dependent dimension of student political activism. Instead of the two-dimensional causal arguments which have been employed to explain the origins of student behavior in the past, a multivariate model appears better suited to test the theoretical assumptions available for verification.

FAMILY STATUS AND SOCIALIZATION

Much of the recent research on family status and socialization suggests that parental socialization is a class-based phenomenon and that the permissive styles of child rearing are essentially middle-class values. Bronfenbrenner (1967), Kohn (1963), and Clausen (1968) all argue that the dominant motif of middle-class parental values centers around the child developing his own standards of conduct and that socially desirable behavior consists of the child acting in accordance with the dictates of his own conscience. Middle-class parents are concerned with the motives and feelings of their children and are attentive to internal dynamics. They rely on indirect psychological techniques of discipline, such as reasoning and appealing to feelings of guilt. Working-class parents, on the other hand, frequently respond to the immediate consequences of the child's behavior and tend more often to physically punish their children for reasons of propriety. Above all, working-class parents do not want their children to violate proscriptions or prescriptions that threaten family respectability (Kohn 1963).

Family socialization practices are affected by location in the social class hierarchy, and these in turn may influence the political attitudes and behavior of youth. But there are other aspects of family position in the social structure which may influence socialization practices, political attitudes, and behavior. Differences in ethnicity, nationality or race, in religion, and in political identification and activity may also affect the course of political socialization.

Youth coming from families whose parents or grandparents immigrated to the United States from low-ethnic-prestige countries, notably from eastern and southern Europe, Latin America, Puerto Rico, and Asia, and recent black mobiles from the South have been forced into precarious minority or marginal group existence which perpetuates and reinforces dependence on subcultural values. Youth born into such homes are socialized into "old country" life-styles, and, depending on the time of arrival to the New World and exposure to American education and other "typically" American institutions, many continue to identify with their ethnic-national traditions. The great majority of the newly arrived ethnic groups are ranked low or near the bottom of the scale of ethnic prestige and political power in

America today, while the traditional settlers are ranked at the top (Baltzell 1964).

Similarly, religious identification and organizations in the United States are ranked according to membership size and degree of perceived prestige —with low-status cults and groups of atheists and agnostics competing with intermediate-status Judaism and Roman Catholicism versus high-status Protestantism. Acceptability into "legitimate" American society today is determined partially by one's professed religious affiliation, with rudimentary institutional authority, visibility, and mobility given to the minority religious groups in a predominantly white Anglo-Saxon Protestant dominated social order. As a result, minority religious groups remain loyal to their socioreligious heritage primarily for reinforcement and identity, which affect both child-rearing practices and attachment to the political community (Baltzell 1964; Lipset 1968*b*, pp. 169–76).

Political affiliation is ranked in the United States in terms of perceived legitimacy and effectiveness. While the American two-party system remains the dominant form of organized politics today, in many instances political power and authority are dependent upon financial support and industrial influence. As a result, socialists, radical democrats, third parties, and right-wing and most other minority political groups are ranked low on the scale of political legitimacy and efficacy, while the Republican business community, old wealth, and to a lesser extent mobile *nouveau riche* Democrats, the latter of whom manage to sustain electoral support, rank near the top of political power in this country. Although liberal and radical elements may occasionally exhibit party and tactical advantages, the balance of power in this political struggle favors the conservatives. As Lipset (1968*b*, p. 314) noted, the center of gravity of wealth and power in the United States is on the Republican side, while the center of gravity of poverty is on the Democratic side.

SOCIALIZATION AND STUDENT POLITICS

The importance of socialization in developing the basic personality patterns and political identification of children is a well-documented area of research in political sociology. Maccoby, Mathews, and Morton (1954) and Middleton and Putney (1963) wrote of family influence on the development of political activity in their progeny. More recently, Keniston (1968) inferred that student political activists on the whole were not rebelling against the values and ideologies of their parents. On the contrary, he found in clinical interviews with a Vietnam Summer group at Harvard University that many of these youth were "living out" their parents' values in practice. Flacks's (1967, pp. 52–75) research reported that political activists indeed were somewhat closer to their parents' values than nonactivists. Solomon

and Fishman (1964, pp. 54–73) suggested that civil rights and peace demonstrators were *ipso facto* "acting out" the values their parents had taught them in the home and by which they expected them to live.

In addition, Keniston (1968, pp. 306–10) and Flacks (1967) described the democratic and equalitarian styles of family decision making in the homes of left-wing student activists. They suggested that the dominant ethos of (leftist) activist-prone families was equalitarian, permissive, democratic, and highly individuated. According to these authors, leftist middle-class families placed a high premium on democratic problem solving, self-expression, intellectual independence, and avowed freedom of dissent. Leftist student activists came from homes where parents shared equally in decision making, where disagreement was openly expressed and negotiated rather than submerged, and where one would expect sanctioned argumentation at family gatherings. As Keniston (1968, p. 310) noted in studying youth from protest prone families.

> We might expect that these will be families where children talk back to their parents at the dinner table, where free dialogue and discussion of feelings is encouraged, and where rational solutions are sought to everyday family problems and conflicts We would also expect that such families would place a high premium on self-expression and intellectual independence, encouraging their children to make up their own minds and to stand firm against group pressure.

Although less empirical evidence supports the hypothesis, it is believed that student political activists coming from conservative homes have been exposed to less democratic, less permissive, and less demonstrative backgrounds than their liberal counterparts (Block, Haan, and Smith 1968).

FAMILY STATUS AND STUDENT POLITICS

The literature on family status and student politics presents consistent findings concerning status characteristics of left- and right-wing students. In previous research, class unequivocally was seen to be related to student politics. Lyonns (1965), Flacks (1967), Paulus (1968), and Kahn (1968) all noted the generally upper-middle-class composition of their left-wing students, while Evans (1961, pp. 42–65) described his right-wing group as coming from lower-middle-class and respectable working-class homes. Although little evidence has been presented concerning the ethnic status composition of student activists, Flacks (1967) and Kahn (1968) made reference to the low-ethnic-status backgrounds exhibited by many of the left-wing students in their respective samples. In addition, Kahn (1968) noted the predominantly white Anglo-Saxon background of the nonactivist students in his research. Religion likewise was found to be related to the direction of student politics. Solomon and Fishman (1964), Keniston

(1968), and Kahn (1968) described their left-wing student sa
having a disproportionate number of students from Jewish, Prote
minority religious backgrounds, while Solomon and Fishman (19
Braungart (1966) noted the strongly Christian religious backgr
their politically conservative student samples. And finally, a rich l
exists suggesting the generational consistency between parents'
affiliation and status and the political profiles and activities
offspring. Lyonns (1965), Keniston (1968), Solomon and Fishman
and others pointed to the politically liberal views of the parents
wing activists, while Solomon and Fishman (1964), Schiff (19
Evans (1961) noted that right-wing activists described their pa
Republican.

FAMILY STATUS, SOCIALIZATION, AND STUDENT POLITICS

Past research on family socialization and student politics has em
the importance of social class as one of the salient factors in
equalitarianism, self-expression, and student political activism, but
this line of reasoning may prove to be spurious since other struc
in our case, family status variables may explain the process equa
As Lipset (1968*a*, pp. 49–51) has cautiously pointed out, while
jority of studies on student political activism are relatively consist
findings are inconclusive and unconvincing at this time because
to hold constant sociological and politically relevant factors in t
grounds of students. For example, Lipset (1968*a*, pp. 49–51) cla
such studies report

> that leftist activists tend to be the offspring of permissive families
> by child rearing practices, and of families characterized by a stron
> who dominates family life and decisions Conversely, conservative
> tend to come from families with more strict relationships betwee
> and children, and in which the father plays a dominant control
> But to a considerable extent these differences correspond to *lit*
> than the variation reported in studies of Jewish and Protestant
> Childhood rearing practices tend to be linked to socio-cultural
> outlooks To prove that such factors play an independent role
> mining the political choices of students, it will first be necessary to
> students within similar ethnic, religious and political-cultural
> ments. This has not yet been done

In response to Lipset's suggestion, we shall consider simultaneo
relationships among aspects of family background and socializati
tices as they affect political identification and activism.

The family serves as a mediating institution situated betw
struggles of society in general and the political socialization pr

contemporary college youth in particular. Family status is established by its social class, ethnic, religious, and political anchorages, which collectively are defined as the *independent* variable(s) in our theoretical model. While there undoubtedly is some overlap and semantic coagulation among these four independent concepts or test variables, it is believed, and empirical evidence supports the view, that each social structural or family status dimension has conceptual autonomy and independent variability (see, for example, the arguments developed by Barber [1961] and Lenski [1954]). Select aspects of socialization, the *intervening* variable(s), are defined through the concept of democratic family structure. Democratic family structure is defined further in terms of parental decision making (equalitarian or democratic versus authoritarian) and the frequency of family political argumentation occurring between the parents and their offspring (or a measure of verbal dissent denoting freedom in the family for the offspring to talk back or freely express themselves when their views conflict with parental views).[2] We realize and admit that our definition of socialization is neither inclusive and exhaustive nor is it indicative of the general literature that exists under the rubric of socialization in sociology. Nevertheless, these select dimensions or aspects of socialization—parental decision making and family argumentation—have been employed in previous analyses of socialization and student politics, and therefore we intend to incorporate these concepts into our definition of family socialization. Earlier findings indicate that families socialize their progeny and provide the basic cognitive, moral, and political values preparing their youth for entrance into adult society. Thus, both independent and intervening variables are conceptualized as influencing the source and direction of student politics—the *dependent* variable in our study—which is defined in terms of student group membership or political identification. When applying this deductive theoretical model to our student sample, the question remains of what linkages exist between select indices of family status and socialization as such factors affect the political behavior of college youth.

[2] The definition of democratic family structure in terms of styles of parental-parental and parental-progeny interactions is synonymous with the term "permissiveness" as it generally has been discussed in the literature. That is, family structure can be either permissive (democratic) or restrictive (authoritarian) between the parents themselves, which has been discussed by Bronfenbrenner (1967), Kohn (1963), Keniston (1968), Flacks (1967), and others, or the parents can be permissive (democratic) or restrictive (authoritarian) with their progeny, as has been suggested by Kohn (1963), Keniston (1968), Flacks (1967), and others. Therefore, the use of the term "permissiveness" has been somewhat ambiguous with repect to the level of interaction that occurs between the parents themselves or between the parents and their progeny. Because of this lack of conceptual clarity, we prefer to use the concept "democratic family structure" in terms of parental level of behavior in addition to parental-progeny level of behavior rather than the general concept "permissiveness," which fails to discriminate between the two levels of family interaction.

(1968), and Kahn (1968) described their left-wing student samples as having a disproportionate number of students from Jewish, Protestant, or minority religious backgrounds, while Solomon and Fishman (1964) and Braungart (1966) noted the strongly Christian religious backgrounds of their politically conservative student samples. And finally, a rich literature exists suggesting the generational consistency between parents' political affiliation and status and the political profiles and activities of their offspring. Lyonns (1965), Keniston (1968), Solomon and Fishman (1964), and others pointed to the politically liberal views of the parents of left-wing activists, while Solomon and Fishman (1964), Schiff (1964), and Evans (1961) noted that right-wing activists described their parents as Republican.

FAMILY STATUS, SOCIALIZATION, AND STUDENT POLITICS

Past research on family socialization and student politics has emphasized the importance of social class as one of the salient factors influencing equalitarianism, self-expression, and student political activism, but in fact, this line of reasoning may prove to be spurious since other structural or, in our case, family status variables may explain the process equally well. As Lipset (1968*a*, pp. 49–51) has cautiously pointed out, while the majority of studies on student political activism are relatively consistent, such findings are inconclusive and unconvincing at this time because they fail to hold constant sociological and politically relevant factors in the backgrounds of students. For example, Lipset (1968*a*, pp. 49–51) claims that such studies report

> that leftist activists tend to be the offspring of permissive families as judged by child rearing practices, and of families characterized by a strong mother who dominates family life and decisions Conversely, conservative activists tend to come from families with more strict relationships between parents and children, and in which the father plays a dominant controlling role. But to a considerable extent these differences correspond to little more than the variation reported in studies of Jewish and Protestant families Childhood rearing practices tend to be linked to socio-cultural-political outlooks To prove that such factors play an independent role in determining the political choices of students, it will first be necessary to compare students within similar ethnic, religious and political-cultural environments This has not yet been done

In response to Lipset's suggestion, we shall consider simultaneously the relationships among aspects of family background and socialization practices as they affect political identification and activism.

The family serves as a mediating institution situated between the struggles of society in general and the political socialization process of

contemporary college youth in particular. Family status is established by its social class, ethnic, religious, and political anchorages, which collectively are defined as the ***independent*** variable(s) in our theoretical model. While there undoubtedly is some overlap and semantic coagulation among these four independent concepts or test variables, it is believed, and empirical evidence supports the view, that each social structural or family status dimension has conceptual autonomy and independent variability (see, for example, the arguments developed by Barber [1961] and Lenski [1954]). Select aspects of socialization, the ***intervening*** variable(s), are defined through the concept of democratic family structure Democratic family structure is defined further in terms of parental decision making (equalitarian or democratic versus authoritarian) and the frequency of family political argumentation occurring between the parents and their offspring (or a measure of verbal dissent denoting freedom in the family for the offspring to talk back or freely express themselves when their views conflict with parental views).[2] We realize and admit that our definition of socialization is neither inclusive and exhaustive nor is it indicative of the general literature that exists under the rubric of socialization in sociology. Nevertheless, these select dimensions or aspects of socialization—parental decision making and family argumentation—have been employed in previous analyses of socialization and student politics, and therefore we intend to incorporate these concepts into our definition of family socialization. Earlier findings indicate that families socialize their progeny and provide the basic cognitive, moral, and political values preparing their youth for entrance into adult society. Thus, both independent and intervening variables are conceptualized as influencing the source and direction of student politics—the ***dependent*** variable in our study—which is defined in terms of student group membership or political identification. When applying this deductive theoretical model to our student sample, the question remains of what linkages exist between select indices of family status and socialization as such factors affect the political behavior of college youth.

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THE STUDENT SAMPLE

One hundred nine college Young Democrats (YD), 117 Young Republicans (YR), 248 members of Students for a Democratic Society (SDS), and 215 Young Americans for Freedom (YAF), representing mainstream and radical positions on both the political Left and Right, were surveyed from ten eastern colleges and universities and from two national samples of SDS and YAF. These four groups were chosen for study inasmuch as they represent the gamut of legitimate and radical political elements encountered on most American college and university campuses today; they are the largest campus-based political activist groups in the United States; and they best articulate the changing political anchorages, cleavages, and conflicts that have emerged with such dramatic force in the 1960s.

During the academic year 1966–67, student political leaders representing the four groups selected for study were contacted; the study was described to them, authorization was secured to survey constituent group members; dates were confirmed for administration of the questionnaire; and local chapters were subsequently surveyed *en masse* periodically throughout the academic year The rationale for selecting the schools included in the sample was based on sheer size of the institution (in terms of enrollment), its urban-rural setting, academic status or prestige, and the funds available for research. The ten institutions selected represent significant differences in size, ecological recruitment base, public or private status, academic ranking, and differential student-body selectivity.[3] Fortunately, two national conventions of SDS and YAF were held during the data-collecting phase of the study (SDS held a national convention at Antioch College, Yellow Springs, Ohio, while YAF held its convention at Franklin and Marshall College, Lancaster, Pennsylvania), and their respective delegates were included in our sample.

While the sample represents student politics in terms of direction and intensity of involvement, it fails to provide a base or anchoring criteria (i.e., nonpolitical student population) with which to compare our political groups. Therefore, an additional sample of 557 introductory-sociology students, that is, students not holding membership in our four groups, was collected for study from the City University of New York, University of Maryland, Pennsylvania State University, and Temple University. This

[3] Pennsylvania State University, Temple University, University of Pittsburgh, University of Maryland, and the State University of New York at Binghamton were chosen for study because they represent large state universities from both urban and rural regions The State University of New York at Binghamton, the exception here, is a medium-size institution The City University of New York and Brooklyn College were included for study inasmuch as they represent medium-size institutions in a major metropolitan area The University of Pennsylvania, Johns Hopkins University, and Carnegie-Mellon University exemplify elitist and specialized institutions with highly selective admission policies

sample of apoliticals, more typical of the college population at large, serves as the control group (CG) for comparative analysis between left- and right-wing student activists.

OPERATIONALIZATION OF THE MULTIVARIATE MODEL

Operationally, family status, which provides the milieu and sets the style for family upbringing, is defined as our independent variable. Socialization, as determined by democratic family structure, is operationalized as our intervening variable, and direction of student political activism is operationalized as our dependent variable. Both independent and intervening variables are employed to predict and explain variance in the dependent variable. For uniformity, nominal and ordinal testing procedures are scaled from low (or low status) to high (or high status), which provides directional consistency in reading both the γ's and correlation-regression coefficients.[4]

Dependent Variable: Student Politics

Student political group membership and identification (X_1).—This, conceptually, refers to the active participation in and/or identification with radical or conventional political behavior of the student while attending college, based on the conceptual scheme of: (1) revolutionary-radical Left; (2) practical Left and liberal center; (3) apolitical center, (4) sophisticated conservative and practical Right; and (5) radical-revolutionary Right.[5] Operationally, these political labels are viewed as homologous with student membership in: (1) Students for a Democratic Society (SDS); (2) Young Democrats (YD), (3) control group (CG) of apolitical non-activist students; (4) Young Republicans (YR); and (5) Young Americans for Freedom (YAF).

In addition, ideological direction of student political identification is scaled from revolutionary Left to revolutionary Right, based on the conceptual scheme corresponding to a subject's identification with: (1) Gus Hall, (2) Mario Savio (revolutionary radicals); (3) Staughton Lynd, (4) Norman Thomas (radicals); (5) Hubert Humphrey, (6) Robert Kennedy (liberals); (7) Walter Lippmann, (8) Harry Truman (conservative liberals); (9) Nelson Rockefeller, (10) Dwight Eisenhower

[4] The highest level of measurement our data can reach is ordinal Therefore, in describing the bivariate relationships, the Goodman and Kruskall γ (Costner 1965, p 350) is employed, while the multivariate treatment of our data utilizes path analysis (Duncan 1966, Blalock 1968; Land 1969)

[5] This political continuum is based on the conceptual schemes of Mills (1948, pp 13–30) and Rossiter (1962, pp 11–14)

(liberal conservatives); (11) Richard Nixon, (12) Everett Dirksen (conservatives); (13) Barry Goldwater, (14) William F. Buckley, Jr. (reactionaries); (15) Robert Welch, and (16) George Lincoln Rockwell (revolutionary radicals).[6]

Intervening Variable: Socialization (Democratic Family Structure)

Parental decision making (X_2).—This, conceptually, refers to style of parental decision making (democratic versus authoritarian) exhibited in the families of our student sample. Operationally, parental decision making is defined as: (1) authoritarian (father or mother makes all decisions in family); (2) authoritarian-democratic (father or mother makes most of important decisions); and (3) democratic (both father and mother share equally in family decision making).

Family argumentation (X_3)—This, conceptually, refers to the frequency of argumentation of offspring with one or both parents over the substantive issues of civil rights, the distribution of wealth, and American foreign policy. Operationally, frequency of argumentation with one or both parents is defined in terms of a composite index score based on the following weightings: 2, often argued; 1, occasionally argued; and 0, practically never argued. The collapsed scale adopted for our purposes consists of: (1) low (composite argument index scores of 0-1); (2) medium (argument index scores of 2–4); and (3) high (argument index scores of 5–6).

Independent Variable: Family Status

Social class (X_1).—This, conceptually, refers to families who share similar life-styles with respect to occupational and educational backgrounds in the social stratification hierarchy. Operationally, it is defined by the Hollingshead Index of Social Position, which combines scores on the Edwards seven-point occupational prestige scale with a seven-point education scale (Hollingshead 1957). For purposes of this analysis, Hollingshead Classes IV and V are collapsed and defined as the working class; Class

[6] When subjects were asked to indicate the general political philosophy closest to their own by identifying with one of these sixteen political personalities, the following group mean scores emerged SDS = 3 11 (Lynd); YD = 6 10 (Kennedy); CG = 7 95 (Lippmann-Truman); YR = 10 13 (Eisenhower); and YAF = 13.41 (Goldwater). The rank-order correlation reliability index of this political identification profile scale, which was developed in 1966, has been predetermined by a series of judges at $r = 95$ While the nominally scaled group continuum of our five student groups is employed for descriptive analysis, the ordinally scaled political identification profile is employed in path analysis Essentially, both group membership and political identification dimensions tap the same phenomenon: direction of student politics. The reliability of these two scaled items with each other is $r = 83$ (significant at .01).

III is defined as the lower middle class; and Classes I and II as the upper middle class.

Ethnicity (X_5).—This, conceptually, refers to groups bound together by similar cultural and nationality ties who are the products of similar historic evolution, social organization, and migration to the United States. Operationally, it is defined as ethnic status relative to geographic or nationality origin of both parents as follows: (1) Low-ethnic-status categories include: English–eastern or southern European, eastern European–German, Irish–eastern or southern European, Italian, eastern European, and Afro-American and/or West Indian. (2) High-ethnic-status categories include the more traditional or earlier immigrants: English, Scotch-Irish, Welsh, German, French, Scandinavian, Dutch, English-German, English-Irish, Irish–northern European, and Irish.

Parents' religious affiliation (X_6).—This, conceptually, refers to formal religious affiliation of one or both parents. Operationally, it is classified into four rank categories from low to high status based on minority-majority membership and institutional dominance in this country: (1) nonreligious, including parents who are agnostics or atheists; (2) Jewish (one or both parents); (3) Roman Catholic or Greek Orthodox (one or both parents); and (4) Protestant parents.

Parents' political affiliation (X_7)—This, conceptually, refers to the organization, party, or political label best articulating the spectrum or political ideologies in the families of our collegiate sample. Operationally, it is scaled from liberal to conservative according to the following categories: (1) radical Left, including radical, socialist, and communist; (2) moderate Left, including Democrats; (3) independent; (4) moderate Right, defined as Republican; and (5) radical Right, including conservative and right-wing groups.

RESEARCH FINDINGS. BIVARIATE AND MULTIVARIATE

Bivariate

The democratic family structure and family status characteristics of our collegiate sample are presented in tables 1–6 in simple bivariate fashion. In tables 1 and 2, we note the differences in parental decision making and argumentative styles of our five student groups. In table 1, we can see there are slight differences in styles of parental decision making and student political activism. The γ of —.1637 is not particularly strong,[7] since students from all groups come from democratic homes, but we can detect a

[7] Strength connotes relative size of each γ in relation to the other γ's for our data. For example, we will consider γ's of .10 and below as low, between 11 and .29 as medium, and 30 or above as high since, in this latter instance, γ's of .30 or higher are rare for our data

TABLE 1

DISTRIBUTION OF STUDENT GROUPS BY PARENTAL DECISION MAKING
($\gamma = -.1637$)

PARENTAL DECISION MAKING	GROUP SDS	YD	CG	YR	YAF	Total
Authoritarian	29	9	66	15	33	152
	(19 1)	(5.9)	(43.4)	(9 9)	(21 7)	(100 0)
Authoritarian-democratic	56	30	178	45	82	391
	(14 3)	(7.7)	(45 5)	(11.5)	(21.0)	(100 0)
Democratic	131	52	268	49	78	578
	(22.7)	(9.0)	(46 3)	(8 5)	(13.5)	(100 0)
Total N	216	91	512	109	193	1,121

TABLE 2

DISTRIBUTION OF STUDENT GROUPS BY FAMILY POLITICAL ARGUMENT
($\gamma = -.2214$)

FAMILY POLITICAL ARGUMENT	GROUP SDS	YD	CG	YR	YAF	Total
Low	60	31	264	48	84	487
	(12 3)	(6 4)	(54 2)	(9 9)	(17.2)	(100 0)
Medium	106	65	224	56	106	557
	(19.0)	(11 7)	(40 2)	(10 1)	(19 0)	(100 0)
High	77	12	36	12	20	157
	(49 1)	(7 6)	(22 9)	(7.6)	(12 8)	(100 0)
Total N	243	108	524	116	210	1,201

noticeable trend with more leftist-oriented students recruited from democratic homes than their conservative counterparts.

In table 2, there appears to be a difference in the frequency of family political argumentation among our five student groups. The γ for this relationship is $-.2214$ (which is higher than the parental decision-group relationship), and we note that left-wing political activist youth argue more frequently with their parents than nonactivists or right-wing youth, keeping in mind the notion that left-wing youth tend to be more vocal and tenacious in voicing their views than nonactivists and unruffled conservatives.

The family status characteristics of our sample are described in tables 3–6. There are social class differences in the composition of the five student groups. As seen in table 3, SDS members are from upper-middle-class homes; conversely, the right-wing YAF are drawn from working-class and lower-middle-class homes. The γ of $-.1999$ is not high, but we have about a 20 percent chance of predicting group membership when we know the

TABLE 3

Distribution of Student Groups by Social Class
($\gamma = -.1999$)

Social Class	Group: SDS	YD	CG	YR	YAF	Total
Working class	38	22	197	25	79	361
	(10.5)	(6.1)	(54.6)	(6.9)	(21.9)	(100.0)
Lower middle class	62	38	185	32	67	384
	(16.1)	(9.9)	(48.2)	(8.3)	(17.5)	(100.0)
Upper middle class	124	42	130	54	57	407
	(30.5)	(10.3)	(31.9)	(13.3)	(14.0)	(100.0)
Total *N*	224	102	512	111	203	1,152

TABLE 4

Distribution of Student Groups by Ethnicity
($\gamma = .3495$)

Ethnicity	Group: SDS	YD	CG	YR	YAF	Total
Low	122	62	250	39	60	533
	(22.9)	(11.6)	(46.9)	(7.3)	(11.3)	(100.0)
High	72	28	172	63	132	467
	(15.4)	(6.0)	(36.8)	(13.5)	(28.3)	(100.0)
Total *N*	194	90	422	102	192	1,000

TABLE 5

Distribution of Student Groups by Parents' Religion
($\gamma = .3882$)

Parents' Religion	Group: SDS	YD	CG	YR	YAF	Total
Nonreligious	47	4	11	5	7	74
	(63.5)	(5.4)	(14.9)	(6.7)	(9.5)	(100.0)
Jewish	102	67	208	24	27	428
	(23.8)	(15.7)	(48.6)	(5.6)	(6.3)	(100.0)
Catholic	23	22	149	22	67	283
	(8.1)	(7.8)	(52.6)	(7.8)	(23.7)	(100.0)
Protestant	67	16	152	63	107	405
	(16.5)	(4.0)	(37.5)	(15.6)	(26.4)	(100.0)
Total *N*	239	109	520	114	208	1,190

TABLE 6

Distribution of Student Groups by Family Politics
(γ = .5075)

Family Politics	Group					
	SDS	YD	CG	YR	YAF	Total
Radical Left	32 (86.5)	1 (2.7)	3 (8.1)	1 (2.7)	0 (0.0)	37 (100.0)
Moderate Left	104 (20.6)	86 (17.1)	265 (52.6)	16 (3.2)	33 (6.5)	504 (100.0)
Independent	69 (24.6)	16 (5.7)	119 (42.3)	20 (7.1)	57 (20.3)	281 (100.0)
Moderate Right	30 (9.2)	5 (1.5)	112 (34.4)	76 (23.3)	103 (31.6)	326 (100.0)
Radical Right	1 (6.2)	0 (0.0)	1 (6.2)	1 (6.2)	13 (81.4)	16 (100.0)
Total *N*	236	108	500	114	206	1,164

social class position of our student sample. It is of interest to note the social class cleavage between the two radical groups in our sample.

There is little doubt when viewing table 4 that ethnicity appears to be related to student political activism, with leftist political activists drawn from low-status southern or eastern European backgrounds, while the right-wing youth come from higher-status northern European backgrounds. The γ for this table is .3495, indicating a relationship between high ethnic status and conservative student political activism. That is, students with low-ethnic-status backgrounds are drawn into left-wing student politics, while youth with high-ethnic-status backgrounds lean toward conservatism.

In table 5, we note an association between parents' religious status and student group membership. The γ for this table is .3882, with youth from low-religious-status homes more likely to be members of left-wing or liberal political groups, while those from higher-religious-status homes are often right-wing or conservative group members.

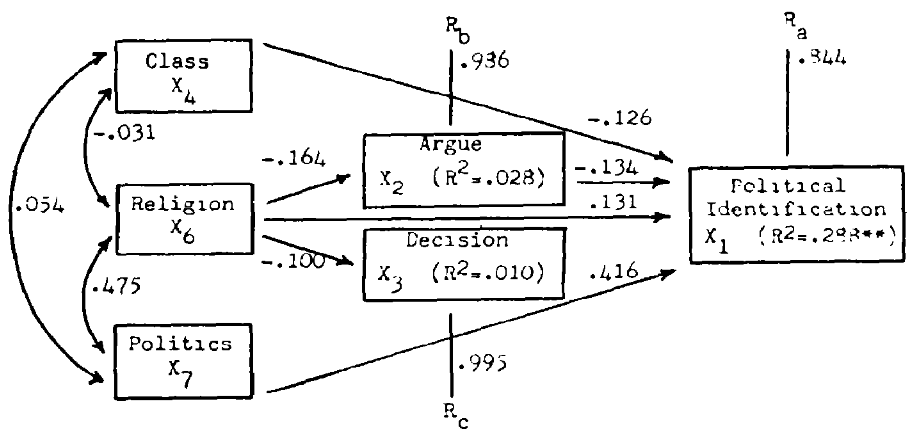
The relationship between family political status and student group membership is the strongest we have encountered thus far, with offspring following the political direction of their parents (see table 6). The γ for this table equals .5075, and it appears that if we know the parents' political identification, we can predict fairly accurately the political affiliation of their offspring (at least 51 percent of the time in a bivariate sense).

Multivariate

Path analysis is employed to test the multivariate relationship among family status, democratic family structure, and student politics in an asymmetric and cumulative sense. Instead of comparing each of the inde-

pendent variables with the intervening and dependent variables, as was the case with γ analysis, path analysis tests the total relationship of the independent variables as they singly and collectively explain variance in the dependent variable. By employing this methodology, we are better able to determine whether a proposed set of deductive propositions prove to be internally consistent in addition to being related theoretically and empirically.

In figure 1, we immediately note that the ethnicity variable is eliminated from the path diagram.[8] A preliminary run for this path diagram indicated a high zero-order correlation between ethnicity and political



Zero-Order Correlation Matrix

	1	2	3	4	6
1					
2	-.195**				
3	-.107**	.055			
4	-.113**	.036	.024		
6	.355**	-.165**	-.100*	-.031	
7	.432**	-.083*	-.093*	.054	.475**

**significant at .01 level; *significant at .05 level.

FIG. 1 —Path diagram showing influence of family status, socialization on student politics for total student group sample ($N = 893$). The variable ethnicity (X_5) appeared to be not related significantly to our intervening and/or dependent variables in our path diagram and therefore was deleted from the model The recursive equations for fig. 1 resemble the following: $X_3 = p_{36}X_6 + p_cR_c$; $X_2 = p_{26}X_6 + p_bR_b$, and $X_1 = p_{12}X_2 + p_{14}X_4 + p_{16}X_6 + p_{17}X_7 + p_aR_a$.

[8] Variables were eliminated from the path diagram if the β's were less than twice their standard errors (Duncan 1966, p 6)

identification ($r = .264$, significant at .01).[9] Even though one would expect ethnicity to be related to the dependent variable, as indicated previously by the relatively high γ, the β coefficients for ethnicity–democratic family structure and ethnicity–political identification were not strong enough to warrant inclusion in the path model. Since the β coefficient "controls out" the effects of all other variables in the system and allows only the independent variable to affect the dependent variable, then, when all other determining variables were controlled ($\beta_{15 \cdot 23467} = .033$), ethnicity lost its explanatory power for political identification.[10] Through the expansion of the general path formula, it was possible to explore the direct effect of ethnicity on political identification along with the indirect effects of the other independent variables on ethnicity and thus determine more precisely which variables led to the attrition of ethnicity from the model. The direct effect of ethnicity (X_5) on political identification (X_1) was negligible (.033), as mentioned previously. However, the total indirect effects on ethnicity–political identification were high (231), with family politics (.166) and religion (.068) having the strongest indirect effects on the ethnicity–political identification relationship. It appears that family politics–ethnicity and religion–ethnicity are highly interrelated, and when these interrelationships were controlled, ethnicity was unable to maintain its strength as a determining variable for student political identification. In light of this "contamination effect," we can assume that the strong γ between ethnicity and student group membership was due to the underlying effects of politics and religion.

The zero-order correlation matrix in figure 1 indicates that the three remaining independent variables and two intervening variables all appear to be correlated significantly with our dependent variable. In addition, when observing the intercorrelations (zero-order correlations) of the independent family status variables—class, religion, and politics—we can see that, while religion and politics are highly correlated ($r = .475$), religion and class ($r = -.031$) and class and politics ($r = .054$) do not appear to be significantly interrelated. Therefore, there does appear to be some inde-

[9] Although ethnicity was not related to decision making ($r = -029$), argument score ($r = -054$), or social class ($r = 070$), it was highly interrelated with the independent variables of religion ($r = 576$, significant at .01) and family politics ($r = .395$, significant at .01).

[10] The direct and indirect effects are computed by the formula $r_{ij} = p_{ij}$ (direct effect) $+ \Sigma p_{iq} r_{jq}$ (indirect effects), when i and j are two variables in the model and q represents all variables from which paths lead directly to X_i. The specific calculations for ethnicity in our model are as follows:

$$r_{15} = 033 + p_{12} r_{52} + p_{13} r_{53} + p_{14} r_{54} + p_{16} r_{56} + p_{17} r_{57},$$

$$r_{15} = 033 + (-119)(-054) + (-036)(-029) + (-128)(077) + (118)(576) + (421)(395).$$

$$r_{15} = 033 + 066 + 001 + -010 + 068 + 166,$$

$$r_{15} = 033 \text{ (direct effect)} + 231 \text{ (indirect effects)}$$

pendence in our family status variables, especially between class and religion and class and politics. Family argument and parental decision making are not significantly correlated ($r = .055$), which implies some independent variation in these two variables.

Generally speaking, about 28.8 percent (significant at .01 level) of the variation in political identification can be explained by the combined influence of family politics ($\beta = .416$), family argument ($\beta = -.134$), religion ($\beta = .131$), and social class ($\beta = -.126$) in the total sample Only 2.8 percent of the variation in family political argument score and 1.0 percent of the variation in parental decision making can be explained by religion, with religion the only family status variable having any effect on democratic family structure. It appears that family religious status has a somewhat greater effect on family argumentation ($\beta = -.164$) than on parental decision making ($\beta = -.100$), and as family religious status decreases, family argumentation and democratic decision making increase.

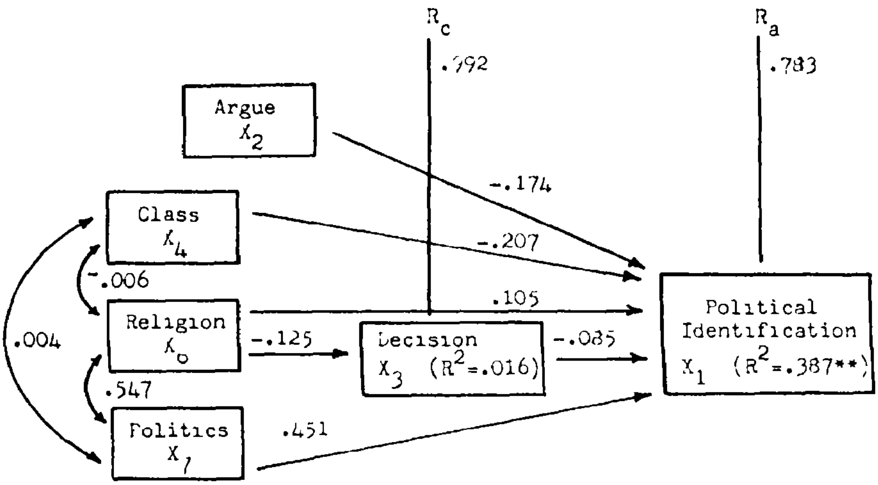
In figure 1, family argumentation depends partially on family religious status for its effect on political identification of college students, while parental decision making does not have any significant effect on political identification. As with the γ's, family politics appears to be the strongest predictor variable influencing student political identification. The other variables in figure 1 have about the same relative effect on political identification, all of which are considerably less than that of family politics The direction of the path coefficients supports our earlier findings that: (1) as social class decreases, political conservative identification increases; (2) as family religious status increases, political conservatism increases; (3) as family political conservatism increases, student conservative identification increases; and (4) as argument score decreases, political conservatism increases. In addition to the direct effects on political identification, the indirect effects were computed for each of the diagram variables.[11] Religion was the only variable plagued by strong indirect effects (.224),

[11] When the direct and indirect effects of argue (X_2), class (X_4), religion (X_6), and politics (X_7) are computed with political identification (X_1), the following relationships emerged. The total direct effect of argue on political identification equaled $-$ 134, while the total indirect effect on argue with political identification was $-$ 061. Politics ($-$ 034) and religion ($-$ 022) had the largest indirect effect with argument–political identification, $r_{12} = -134 + -061$ The direct effect of class on political identification was $-$ 126 The total indirect effect on class–political identification (.013) was influenced mainly by politics (022), $r_{14} = -126 + 013$ The direct effect of religion on political identification was 131, while the total indirect effect on religion–political identification (224) was greatly influenced by politics (198), $r_{16} = 131 + 224$ The direct effect of politics on political identification was 416, and the total indirect effect on politics–political identification (066) was influenced by religion (062), $r_{17} = .416 + 066$. When explaining the path equation to determine the indirect effects, we observe the strong underlying effects politics and religion exert on the variables in our model

which were due primarily to the influence of family politics on religion (.198).

We can surmise from figure 1 that the family status variables of class, religion, and politics do not explain much variation in the socialization indices of family argument and parental decision making, but the combination of the independent and intervening variables explains a substantial and statistically significant amount of variance in the dependent variable. Religion appears to be the strongest family status predictor of democratic socialization, while family politics appears to be the strongest predictor of student political identification.

When comparing the path diagrams for the activist groups, figure 2,

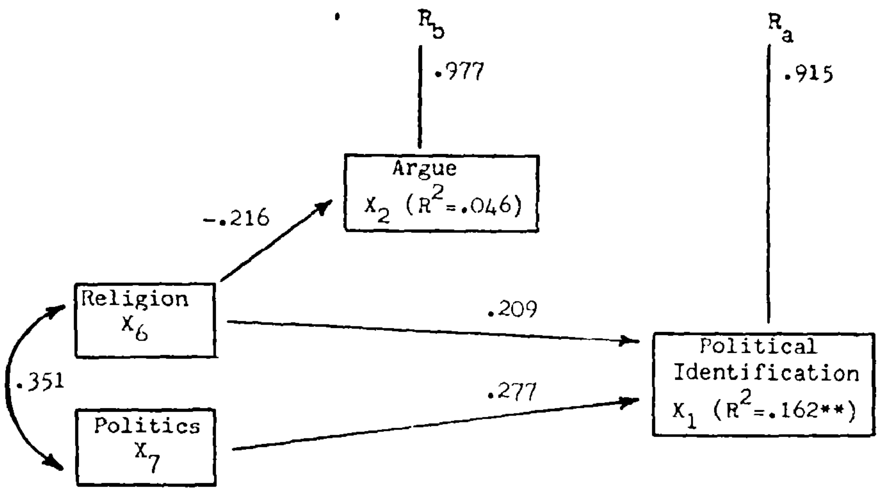


Zero-Order Correlation Matrix

	1	2	3	4	6
1					
2	-.253**				
3	-.171**	.094*			
4	-.206**	.010	-.015		
6	.384**	-.115*	-.125*	-.006	
7	.542**	-.137*	-.133*	.004	.547**

**significant at .01 level; *significant at .05 level.

FIG. 2.—Path diagram showing influence of family status, socialization on student politics for student activists ($N = 528$). The variable ethnicity (X_5) appeared to be not related significantly to our intervening and/or dependent variables in our path diagram and therefore was deleted from the model. The recursive equations for fig. 2 resemble the following: $X_3 = p_{36}X_6 + p_c R_c$; and $X_1 = p_{12}X_2 + p_{13}X_3 + p_{14}X_4 + p_{16}X_6 + p_{17}X_7 + p_a R_a$.



Zero-Order Correlation Matrix

	1	2	6
1			
2	-.084		
6	.307**	-.216*	
7	.350**	-.052	.351**

**significant at .01 level; *significant at .05 level.

FIG 3—Path diagram showing influence of family status, socialization on student politics for control group ($N = 396$) The variables of family decision making (X_3), social class (X_4), and ethnicity (X_5) appeared to be not related significantly to our intervening and/or dependent variables in our path diagram and therefore were deleted from the model The recursive equations for fig 3 resemble the following: $X_2 = p_{26}X_6 + p_bR_b$, and $X_1 = p_{16}X_6 + p_{17}X_7 + p_aR_a$

and control group, figure 3, we are able to explain 38.7 percent (significant at .01 level) of the variation in political identification for the activist groups and only 16.2 percent of the variation in political identification for the control group. Ethnicity was not a significant variable included in the path diagram for the activist group, while ethnicity, class, and decision making were eliminated from the path analysis in the control group. The activist model more closely resembles the path diagram for the total student sample in that an intervening variable provides a causal linkage between the independent and dependent variables, but unlike the total student sample, the crucial intervening variable for the activists is parental decision making rather than argument score. An intervening variable does not provide a causal linkage for the control group.

Family politics appears to be the strongest predictor variable for student political identification for both activist and control groups, albeit a stronger predictor in the path diagram for the activists ($\beta = .451$) than for the control group ($\beta = .277$). The reverse appears to be true for family religious status, with religion a stronger predictor of political identification for the control group ($\beta = .209$) than for the activist youth ($\beta = .105$). Religion affects family argumentation and not parental decision making for the control group ($\beta = -.216$), while religion affects parental decision making ($\beta = -.125$) but not family argument scores in the activist group. We can see that religious and political backgrounds are the only relevant variables explaining political identification in the control group, with much of the variation unexplained.

In addition to the family status variables of religion and politics, several other variables have a significant asymmetric effect on the political identification of the activist group. For the activists, aside from family politics, social class is the next strongest predictor of political identification ($\beta = -.207$), and as social class increases, leftist political identification increases. Argument score, which was not relevant to political identification in the control group, emerges as the third highest predictor of political identification in the activist group ($\beta = -.174$), where, as family argument over politics decreases, conservative political identification increases. Family religious status is the fourth highest predictor in the path diagram ($\beta = .105$), with high religious status positively correlated with conservative political identification. And finally, parental decision making, as influenced by religion ($\beta = -.125$), has a slight effect on political identification ($\beta = -.085$), with youth from low-religious-status homes having parents who share equally in decision making, indicating, more likely than not, leftist political identification.[12]

[12] When exploring the direct and indirect effects of the variables in figs 2 and 3, we note the following relationships. For the control group, the direct effect of religion on political identification is 209, while the indirect effect of politics on religion–political identification is 097, $r_{16} = 209 + 097$ Similarly, the direct effect of politics on political identification is 277, while the indirect effect of religion on politics–political identification is 073, $r_{17} = 277 + .073$ For the activists, the direct and indirect effects are as follows. The direct effect of argue on political identification is — 174, while the indirect effects of decision, class, religion, and politics on argue–political identification is — 084, which is due primarily to the indirect effect of politics on argue–political identification (—.062), $r_{12} = -.174 + -084$ The direct effect of decision making on political identification is —.085, and the indirect effect is —.086, again due mainly to the influence of politics on the decision–political identification relationship (— 060), $r_{13} = -085 + -.086$. The direct effect of class on political identification is — 207, while the total indirect effects on class–political identification are almost negligible (.001), $r_{14} = -.207 + 001$. The direct effect of religion on political identification is 105, and the indirect effects of the other variables in the model on this relationship are substantial (279), primarily due to the influence of politics (247) and argument score (.020), $r_{16} = .105 + .279$ And finally, the direct effect of family politics on po-

Path analysis allows us to observe the total effect the independent variables exert on the intervening and dependent variables. In the case of the activist group, the net differential of explained variance among the independent, intervening, and dependent variables is substantial, while the same relationship for the control group is less impressive. In both instances, however, politics appears to be the strongest predictor of group identification, followed by class, argument, religion, and decision making for the activist group and religion for the control group. In terms of predicting democratic family structure, for all three path diagrams, religion appears to be the only independent variable influencing argument score for the total student group and control group path diagrams, while it is the sole predictor of parental decision making in the activist model. Therefore, while religion provides the strongest influence of all the family status variables on democratic family structure or family socialization,[13] politics provides the strongest explanatory argument for political identification in our sample.

CONCLUSION

Social class, religion, family politics, and family argumentation appear to be related to student activism, as other research has suggested, but by forcing the family status, socialization, and student politics variables into a multivariate analysis, we begin to perceive more clearly that these factors are causally related in a particular way. For the total student sample, family politics, argumentation, religion, and social class, in that order, affected student political identification, with family politics by far the strongest predictor variable explaining variation in student political identification. Such findings indicate that the study of student politics in the past has overemphasized social class as being one of the most crucial determiners of democratic family socialization and student political group membership or identification when, in fact, this may not be the case. In terms of causal priority and amount of variation explained, social class appeared to be less important than family political and religious status in

litical identification is 451, while the indirect effects of the other variables in the model on this relationship are 091, with religion (057) and argument score (024) contributing the majority of the indirect effects, $r_{17} = 451 + 091$ Therefore, we can note, as with the total sample, that most of the indirect effects are due to the influence of family politics on the variables in the model for both the control group and activist group path diagrams.

[13] While religion alone is able to explain variance in our intervening socialization variables, the amount of variance explained is minuscule when compared with the total explained variance for the dependent variable in all three path diagrams And further, religion and politics are highly interrelated in all three diagrams ($r_{67} = .475$ for total sample; $r_{67} = 547$ for activist sample, and $r_{67} = 351$ for control group sample), which suggests that the strength of the independent variable, religion, on indices of socialization is partially due to the influence of politics on religion

our research. What we are suggesting here is that a reevaluation of the strengths of the independent variables may be in order if we are thinking in terms of developing a reliable and valid theory explaining and predicting student politics. In view of this discussion, it appears that future inquiry should focus on the value-orienting factor of parents' religious affiliation as it affects patterns of family socialization, while further investigation might also be undertaken into the area of family political socialization as it influences the intensity and direction of student political activism.

When path analysis for the total student sample was controlled by activists versus nonactivists, the following results emerged. First, the variables in the activist model were able to explain more variation in student political identification than was the case for the total student and control path diagrams. Again, ethnicity was not considered a significant independent variable and was dropped from both the activist and the control group diagrams. The activist path diagram more closely resembled the total student sample in that politics, class, argumentation, and religion all directly affected political identification, but, unlike the total sample, parental decision making rather than family argument proved to be the more significant intervening socialization variable influenced by religion. Politics appeared to be the strongest determiner of student political identification in the control group, followed only by parents' religion. And interestingly, religion again was the only independent variable influencing family argument for the control group

The sequentially related components of this multivariate analysis provide us with a parsimonious and predictive model for what could be called our "middle range" theory explaining family status, democratic family structure, and student politics. This is an improvement over the bivariate analyses that have been employed to explain student politics in the past Structurally, our theory resembles the path diagram in figure 1, which states that:

1. Direction of student politics in a sample of SDS, YD, CG, YR, and YAF members ($R^2_{1\ 2467} = .288$) is determined by parents' political identification ($p_{17} = .416$), family political argumentation ($p_{12} = -.134$), family religious status ($p_{16} = .131$), and social class ($p_{14} = -.126$), in that order, plus the variation that cannot be explained by the variables in the system ($R_a = .844$), or:

$$X_1 = p_{12}X_2 + p_{14}X_4 + p_{16}X_6 + p_{17}X_7 + p_aR_a.$$

2. Family political argument score ($R^2_{2\ 6} = .028$) is influenced by family religious status ($p_{26} = -.164$) plus the unexplained variation ($R_b = .986$), or:

$$X_2 = p_{26}X_6 + p_bR_b.$$

3. Parental decision making ($R^2_{3\cdot6} = .010$) is affected by family religious status ($P_{36} = -.100$) and the variation unexplained ($R_c = .995$), or:

$$X_3 = p_{36}X_6 + p_cR_c.$$

The theory also predicts that religious status affects parental decision making and family argument score, with religion and argument affecting student politics in an additive sense, and that family politics, religion, and class directly affect the direction of student politics.

While our theory explains 28.8 percent of the variation in student politics, the majority of the variation remains unexplained. There are certain possible reasons why we are not explaining more variation in our intervening and dependent variables: (1) measurement error due to sampling bias; (2) imprecise operationalization of our indices; and (3) the exclusion of significant variables from our analysis. Hopefully, future replication of our findings will provide the answers to these questions.

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Commentary and Debate

ON LIEBERSON'S STUDY OF MILITARY-INDUSTRIAL LINKAGES: A FAN LETTER WITH IMPLICATIONS

Once in a rare while a sociologist writes a paper that constitutes a paragon of scientific writing, one that we can use as a model for graduate student instruction. Such a one is Lieberson's study of military-industrial complex which appeared in the January 1971 issue of this Journal. To begin with he deals with a subject of major social importance—is there such a thing as a military-industrial complex in the sense described by the "elitists," or is the case stronger for the pluralists? Presenting the strongest case he can muster for the elitists, with data on circulation of personnel, coordination among corporations, and major military contractors, he finds those data insufficient for the elitist hypothesis. He considers further the extent to which industry depends on a war economy, using military contracts, a regression analysis of military and nonmilitary expenditures in relation to total corporate income, and an economic analysis using Leontieff input-output ratios. Results remain inconclusive for either the elitists or pluralists. He then turns from such cross-sectional analysis to an analysis of historical trends, reaching again the conclusion that neither approach is consistently supported. He turns at last to an alternative perspective, called "compensating strategies," which explains how this could occur without any conspiracy or even community of interest, illustrating the perspective with data on the makeup of U.S. Senate committees.

Lieberson's theory of "compensating strategies" has implications beyond those he describes. In sum, the argument of that theory is that when a small number of firms has a strong interest in some sector of the economy (interest of certain big corporations in military contracts) they can pursue them (without conspiracy or even coordination) provided that the individual loss to any of the other firms is small and it can be argued that there is no net loss. Although Lieberson develops the theory as alternative to the two other theories of military-industrial linkages, it also constitutes an alternative theory to the two major existing theories of decision making, namely, the synoptic or systems-analytic (Archibald 1970) and the incremental (Lindblom 1965, especially chap. 9). The synoptic theory is the classic theory of decision making whereby an individual first examines his objectives or values in some preference order, surveys all possible means for attaining his objectives, exhaustively examines the probable consequences of employing each of the possible means, and chooses a means that has the highest probability of achieving a maximum of his values or objectives.

Incremental theory criticizes synoptic theory as not corresponding to the realities of limited information and motivation of the average decision maker. Following Herbert Simon's classic work on satisficing (as opposed to optimizing), this theory assumes that persons act on the basis of a limited scanning of the field until a possible solution to a problem comes up. That solution, if workable, is adopted without further search. It is possible that further search would turn up a better solution, but further search would itself be costly and the solution might not be sufficiently better to pay for the cost of that search itself.

Lieberson's "compensating strategies" theory provides a third theory For here, it is assumed that those that have major stakes in the outcome will assume the lead in search for a solution that will benefit them highly Others will go along with such a solution provided that none of them loses heavily (if so they will be part of the group with major stakes) and that the net gain is felt to be greater than any alternative available at the moment. The theory is more useful in that it is uniquely sociological, requiring that persons take one another into account (whereas the synoptic or the incremental theory is modeled on the isolated individual, or even a computer, though they can both be applied to group situations). As such, Lieberson's theory, when applied to decision making, allows for group norms, or structured relationships among participants which foreclose certain alternatives and favor others. More important, Lieberson's theory does not *require* that there be any coordination at all among the participants, and certainly no single decision maker who decides what is the optimum solution.

The theory has implications for discussion processes in bodies of peers (such as sociology department meetings or meetings of democratic political bodies). Among senators in state legislatures or at the national level, the phenomenon of senatorial courtesy is, in part, a time-honored name for this strategy. Here the assumption is that if a senator (or group of them) has a strong interest in a particular piece of legislation, other senators will support it provided that they will not be hurt badly, as a group. In similar manner, we find that in a department meeting, persons who feel strongly about some issues, and say so, will carry others who are mildly opposed The result may be that issues that would lose in a mail ballot may be passed.

The theory of compensating strategies bears a close resemblance to the economic concept of a Paretian optimum (defined, in negative form) as follows: "the optimum has not been reached if any reorganization is possible which will make some people better off without making anyone worse off" (Boulding 1955, p. 813). Recognizing that the restriction (no one shall be worse off) is a consequence of insisting on mathematical rigor, if that restriction is slightly loosened we come close to Lieberson's theory.

We can posit three types of cases: (1) The case where some participants gain and no one loses (the Paretian optimum). (2) The case where some gain and some lose, but the net gain to the whole collectivity is equal to or greater than zero. We might call this a collective optimum and look for it in situations of bargaining where some persons have the right to accept settlements on behalf of a collectivity. (3) The case where some gain and some lose, but the distribution is either of the following: a few large gainers and many small losers, or a few large losers and many small gainers. For this case, we can envision two types of situations: (*a*) where the many small losers feel that the effort to overcome their losses will not yield as much as that same effort expended in pursuing a different set of objectives (this is the case Lieberson emphasizes). For the situation of many small gainers, they recognize also that the small gains are not worth enough to them when compared with the serious losses suffered by a few, (*b*) where the many small losers decide that such losses may be worth something in future reciprocity exchanges; that is, as a future claim on the support of the winners in the present dispute (the familiar case of log-rolling). Similarly, the many small gainers would make a similar judgment that the small gain is too small compared with what they might gain in the future with the possible support of those who would lose in the present dispute without their support. Hence, for case (3), the final decision is made in favor of the few large gainers or the few large losers.

Case (3) should not be considered an optimum in that one can only argue that there has been a net gain (or no net loss); that is, the many gainers or losers merely make a judgment that the solution is "best" or "best for the time being." Hence, we can speak of it simply as the Lieberson Solution. It is, however, likely to correspond to reality more commonly than either of the optima.

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AMERICAN CAPITALISM AND MILITARISM: A CRITIQUE OF LIEBERSON

I am sure that all sociologists, both radical and nonradical (elitist and pluralist?), welcome Stanley Lieberson's 1971 article, "An Empirical Study of Military-Industrial Linkages." For too long, scholarly debate provoked by investigations of this type has been absent from the literature. While I welcome the article, I must nevertheless take issue with much of it.

First, two rather minor objections: Lieberson states that "efforts to untangle the conflicting theories often appear more like ideological debates than contributions to scientific knowledge." He says that "scholarly positions on this subject must be judged by their ability to develop a comprehensive system that takes account of the available information rather than by their implications for contemporary politics." Implicit here is the old contention that sociologists can and must separate their personal and/or political lives from their scholarly endeavors. Also implicit is the other old chestnut that sociologists can and must be "value free" or "value neutral." This position, I submit, is not only impossible but also undesirable. The rhetoric of so-called scholarly objectivity constitutes mystification in the social sciences and hinders clarity. We all have our biases and must be aware of them. From that point on we can proceed as fairly as possible.

A second minor point: I find Lieberson's references to Baran and Sweezy objectionable. He contends, "No single research effort can be sufficiently comprehensive or critical . . . to provide a conclusive test of the Marxist argument that capitalism generates a surplus that can only be used up through war and waste." Some elaboration is required. Indeed, the onus is on Lieberson to show why Baran and Sweezy do not "provide a conclusive test" of the Marxist argument vis-à-vis the wasteful and militaristic use of capitalism's generated surplus.

Turning to more substantive criticisms, Lieberson's use of the available literature is questionable—he seems to be unfamiliar with it. A few examples should suffice: Harry Magdoff's *The Age of Imperialism* (1969, especially chap. 5), and "Militarism and Imperialism" (1970); articles by Joseph D. Phillips (1969) and Charles E. Nathanson (1969) entitled "Economic Effects of the Cold War" and "The Militarization of the American Economy," respectively. Five other works of some use are Victor Perlo's *Militarism and Industry* (1963); Fred Cook's *The Warfare State* (1962); and Seymour Melman's *The Defense Economy* (1970), *Our Depleted Society* (1965), and *Disarmament: Its Politics and Economics* (1962).

One of the major problems of Lieberson's article is the kind of evidence

he presents to support his claim that the largest industrial corporations of America derive only a small portion of their total business from primary military contracts (pp. 158–59). His conclusion is based on some dubious ratios, dubious not because they are untrue but simply because they are largely irrelevant. Lieberson took the sales figures of the large corporations. He found that all but three of the fifty largest corporations have a ratio of less than .5, with thirty-three of those fifty having a ratio of less than .05. The importance of this ratio is questionable. In the first place, Lieberson ignores the primary motive of capitalist corporations—the long-run maximization of profits. It is the profitability of such military contracts which is most important.

Magdoff describes the low risk of such military contracts; the safety of such investments is "reliably assured." He writes, "The military-goods market usually has the decided advantage of supplying long-term contracts, often accompanied by enough guarantees to reduce and even eliminate any risk in building additional plant equipment, plant and equipment which may also be used for civilian purposes" (1969, p. 190). Magdoff also notes that such large firms have high production costs with no profits being made up to a certain point of production, that is, the break-even point. Sales beyond the break-even point account for a disproportionate bulk of the profits. Thus, military sales take on an increased relative importance.

Nathanson also points to the profitability of military production. He writes, "Military goods and components are highly specialized and tend to be in relatively scarce supply; they are rarely sold in the civilian market and therefore have no standard market price. All these factors lead to higher profits on military items than on civilian items" (1969, p. 208). Furthermore, the heavy goods industries, such as iron and steel, are slow growth industries, thus making the military factor in their production of crucial importance. Lieberson, then, has overlooked the critical aspect of military contracts.

I suppose that one could also make the point vis-à-vis Lieberson's ratio that the military sales figure is taken over the *total* sales figure, that is, the former is also included in the latter. This would mean, for example, that if the military contract was worth $10 million and total sales were $30 million the resulting ratio would be 1 to 3 or .33. However, if the $10 million figure was taken over the total sales figure minus the military sales figure, then we would get a 1 to 2 ratio or .50. In other words, even if Lieberson's approach in measuring the importance of military contracts to large firms was justifiable, which it is not, then his figures would still be biased in the direction of low ratios (i.e., low importance).

Nathanson's statements about the importance of military production to America's largest firms run opposite to Lieberson's. Nathanson writes,

"I found that AT LEAST 205 of the top 500 corporations are significantly involved in military production, either through their primary industry of production, through diversification into the defense sector, or through military research and development contracts. If we exclude from the top 500 the large number of food, apparel, and tobacco firms, then military production involves about 50 percent of the major firms in the economy. Among the top 100, there were 30 to 35 corporations that probably have no direct involvement with the military, while there were 39 for whom military demand consumes 1–10 percent of the total output, 11 for whom it consumes 10–50 percent of total output, and 14 for whom it consumes over 50 percent" (1969, pp. 231–32, emphasis in original). Even if we ignore Nathanson's work and accept Lieberson's original analysis based on his ratio figures we would still find his study inadequate. Assuming that Lieberson's figures are important and useful we find that he does not relate them to the American capitalist economy as a whole, at least in a way which is particularly meaningful. Take for instance his ratio figure for the large corporate concern of General Dynamics—.993—which appears on page 568. Obviously a sharp cutback in military contracts would send that company reeling and have profound repercussions on the rest of the economy as well (see Magdoff and Sweezy 1970).

Lieberson himself finds his investigation "hardly conclusive" (p. 569). He also makes mention of the indirect effects or benefits of military contracts for other large corporations. For instance, he concedes that there may be indirect benefits to steel mills from which armor plating is purchased. Of course these multiplier effects are quite important for understanding the full impact of military spending. They stimulate sales and employment in economic sectors not actually holding military contracts. Magdoff writes, "It has been estimated that for every $1 spent on national defense, another $1 to $1.40 of national product is stimulated. If we accept only the lower estimate, and assume for the sake of argument equivalent labor productivity in the military and civilian sectors, we reach a measure of unemployment (if there were no military sectors) in the neighborhood of 24.3 percent" (1970, pp. 10–11). Such an unemployment figure is almost equivalent to the unemployment rate of 24.9 percent at the depth of the depression in 1932.

Lieberson also fails to make clear whether he includes seemingly nonmilitary production in his figures and analysis. Industries involved in production of computers, space technology, and research and development are closely allied to militarism. This production, of course, has a further impact upon the American economy.

An interesting table on page 570 supposedly shows that government spending geared to nonmilitary production is more closely linked to corporate income. Lieberson says that "military expenditures have less

impact on corporate income than does an equivalent amount spent by the government on nonmilitary items." This conclusion is naturally subject to the above remarks as well as to this observation—the statistics cover the historical period 1916–65! The importance of military spending, as well as other government spending, to ease depressions and stimulate economic growth are recent, post-World War II phenomena. Only since Keynes and the 1929 crash has such government involvement been acceptable to corporate capitalism. Recognition of this new phenomenon is implicit in Lieberson's article when he notes that military spending does not noticeably cease after World War II as it did after other wars (p. 573). To use data since 1916 seems to me to be a fallacious approach to serious investigation and analysis. I might also point out that such government spending, military and nonmilitary, does not conflict with the private sector and, indeed, stimulates that sector.

Finally there is the question of the importance of the military in protecting American assets abroad as well as providing and opening up markets for trade and investments. Lieberson notes this when he remarks that military spending "provides an umbrella for foreign investments" (p. 571). Such an important impact of American militarism on the U.S. economy cannot simply be noted in passing, however. Magdoff has compiled figures to show that for all industries, except farm machinery and equipment, export and military demand account for 20–50 percent of the total (1969, p. 187). He claims that such production cannot readily be shifted elsewhere as long as capitalists are setting the economic priorities of profit, growth, and security for invested capital (p. 189).

In conclusion, then, although the Lieberson article is useful in stimulating discussion and research on the impact of military spending on the American economy, it is subject to serious criticisms. There is an apparent unawareness of the available literature, a dubious use of certain figures, and an absence of a holistic analysis vis-à-vis militaristic spending.

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LIEBERSON REPLIES TO STEVENSON

Since the paper in question will stand or fall on its own merits, despite Mr. Stevenson's shove, my first impulse after reading his letter was to ignore it. However, the dubious forms of logic, peculiar attitudes toward data, lapses in thought processes, and distortions embedded in the letter call for some comment. In doing so, I will cover the various specific criticisms of the paper at hand.

The first of what Stevenson calls his "two rather minor objections" deals with my assertion that scholars must try to be objective and work with the available information rather than their ramifications for contemporary polemics. This is condemned in one sentence as an "old contention" and later as an "old chestnut," as if the age of a position provides significant evidence for evaluating its validity or usefulness. I suppose the argument that the earth is round rather than flat must by now be an "old chestnut," but I would neither condemn nor praise its advocates for this.

All of this, however, is incidental to the letter's claim, "The rhetoric of so-called scholarly objectivity constitutes mystification in the social sciences and hinders clarity." Stevenson is quite correct in feeling that efforts at scholarship can muddy the issue. For scholarship, by definition, requires considering all of the evidence, pro and con. In polemics, whether it be a political argument or a lawyer representing his client before a jury, one does not try to present both sides but only his own in as masterful a fashion as possible. As a consequence, the complex becomes simple and the unknown is glossed over. The paper in question points out a variety of ways in which military-industrial linkages work to the detriment of the society's general goals. However, as far as I could tell, the results I gathered fail to fully support either the pluralist or the elitist theories.

Stevenson argues, and I would not disagree, that we all have our biases and must become aware of them. However, it is one thing to conclude that mankind's imperfection gives one the right to give up trying; it is another to conclude that we do the best that we can. The argument that it no longer matters how close we come to achieving this goal since it is not

fully attainable is in my opinion bound to create serious difficulties. If Stevenson's letter is an illustration of how the absence of "scholarly objectivity" opens our eyes to what is really going on, I fear that careful examination of his points may well create unanticipated problems for his position.

As for the "second minor point," namely my passing reference to Baran and Sweezy, it is true that I did not take the effort to show why they fail to provide a conclusive test of their positions. However, since the letter never cites either author again even when trying to present some *real* data on the subject at hand, perhaps Stevenson and I are not too far apart in our judgment of their empirical relevance for the questions under consideration.

Enough of this. Let us turn to the basic research questions raised in the letter. When confronted with empirical data that appear contrary to the conclusion you want, one of the neatest tricks is to show that the data are irrelevant. As we shall see, this is not always good enough. Thus my paper used yearly data from 1916 through 1965 to get at the relative influence of federal military and federal nonmilitary expenditures on corporate income after taxes. My findings suggest that nonmilitary spending generates more corporate income than does military spending and is also more closely correlated with the dependent variable. These results are unacceptable to the letter writer—frankly, they were surprising to me. But there is all the world of difference between *surprise* and *acceptability*. It is argued in the letter that the time span covered is inappropriate because certain theories are only with us since Keynes and the 1929 crash. Accordingly, it is claimed that the practice of using military spending to boost the economy is only a post–World War II phenomenon. Why the alleged changes in motivations for spending should affect the correlations and regressions is not entirely clear to me.

But for the sake of argument, let us suppose Stevenson is correct in claiming that this relationship would not hold up in the post–World War II period. All the letter writer would have to do is compute the original variables for the twenty-year period between 1946 and 1965. Since there are three variables, this would mean working with a total of sixty numbers. The references in table 4 are rather detailed, giving not only the sources and pages for the original data, but even the columns that were used. But why should Stevenson recompute the data? What value are the results anyway? If they are consistent with my findings for the longer period, then obviously there is still something wrong with the procedure. If the results collapse, then this is something Stevenson knew anyway. So certainly it makes better sense to reject the results rather than to recompute for the briefer period that Stevenson claims would be appropriate. Actually, I advise the letter writer to make these computations only in

seclusion since I am afraid that the pattern reported in table 4 holds up even for the post–World War II period.

One of the key criticisms pertains to the use of a ratio between military contracts and total sales for the 100 largest industrial corporations. My goal here was to consider the relative importance of military contracts for these corporations. Ideally, one would like to determine the contribution of military production to corporate income—or at the very least, sales—but to my knowledge such data are not available. Accordingly, this ratio was used along with the results from input-output studies and my regression analysis to get some notion of the role of military contracts in large corporations and the economy generally. My results indicate that the ratio of military contracts to toal corporate sales in the late sixties is rather low for a number of major industrial companies, but high for some others. Even after considering input-output analyses, it appears that many industries do not have an enormous stake in military expenditures. But this is heresy. I am saying that there are shades of gray.

First, the ratio itself is criticized, but only after it is changed from one actually used in the paper into a ratio using military sales rather than military contracts. After some arithmetical exercises, the letter writer is able to show that the military proportion of total sales is not the same as the ratio of the two variables. At least here we can agree; any time Stevenson wants to argue that a ratio and a proportion are not the same, I shall be glad to back him up. Alas, none of this has anything to do with the computations in question since the paper clearly indicates that military contracts rather than military sales are used. Since many contracts are awarded for a longer period than a year, one could offer an argument that the method used, if anything, overstates the role of the military as consumers.

But we need not quarrel here over technical matters since the letter provides a lengthy quotation from Nathanson which actually supports my findings. First, Nathanson claims that from thirty to fifty of the largest 100 corporations have no apparent direct contacts with the military. An additional thirty-nine have from 1 to 10 percent of their total output directed to the military. In short, Nathanson finds that as many as seventy-four out of the 100 largest corporations have 10 percent or less of their business directed to the military. Looking at table 3 of the paper in question, seventy-eight out of 100 corporations have ratios of .10 or less. In other words, if Stevenson actually looked at the results in question, he would find that Nathanson's report in no way alters the conclusion made. But there is a difference: my paper is called, "An Empirical Study of Military-Industrial Linkages"; Nathanson's has the title, "The Militarization of the American Economy."

But more than that, Nathanson's research design appears unequaled in

its vagueness. If the quotation used from his paper had begun just one sentence earlier, we would have learned that he uses sources which do not in any systematic way provide the numerical information needed for the claims Nathanson makes. I can say this with confidence since I looked at them myself when conducting this study. All Nathanson claims is that he "tried." Indeed, no criteria are given for determining how he decides when one of the leading corporations is "significantly involved in military production." So the moral seems to be somehow that it is better to come up with the same finding through vague procedures rather than through explicitly stated means. Moreover, if your findings point to a given interpretation that you do not like, just ignore them.

The letter claims that the study fails to include the indirect effects of military spending on the economy—for example, the purchase of armor plate from steel mills by a fabricator with a military contract. Likewise, the contention is advanced that industries such as the computer, space technology, and research and development fields should be considered and were not. All I can offer for an answer, I am afraid, is the trite cop-out, "read the paper, please." If Stevenson does, he will find the section on input-output analysis beginning on page 571 deals exactly with the question of indirect benefits to various companies from military spending as opposed to nonmilitary expenditures of the same amount by the federal government. As a matter of fact, the figures for several of the industries explicitly raised in the letter are actually given at the top of page 572. (In all fairness, it should be noted that was the eleventh page of the article.)

Another rhetorical device that can be used with great effectiveness is to blast away with unverified assertions that sound like empirical truths. This is demonstrated by the letter's claim that the primary motive of capitalist corporations is "the long-run maximization of profits. It is the profitability of such military contracts which is most important." What information can be used to examine this? Does it mean that corporations deeply dependent on the military have relatively stabilized earnings? One gets this impression later on. Since Stevenson cites General Dynamics as a corporation that could not receive a sharp cutback in military contracts since "it would send the company reeling and have profound repercussions on the rest of the economy as well," I trust he would not mind my reporting the net earnings for 1960 and 1961. Respectively, these are losses of $27 million and $143 million (before tax credits, the losses are $62 and $168 million). My point in presenting these figures is not to disprove the letter's contention with a single case, since that is impossible. As a matter of fact, I have no idea whether it is correct or not. Rather, I merely suggest that if we want to determine the validity of these assertions, then the empirical implications must be examined whenever possible.

The letter's discussion of the possible relation between military spending

and overseas investment nearly drove me up the wall. Stevenson's claim that Magdoff's study deals with all industries is probably an example of the "new chestnuts." In the copy of the book that I looked at, Magdoff claims to be dealing only with "the industries producing nonresidential investment goods" (1969, p. 187). Thus Stevenson's quote from Magdoff about export and military demand accounting for 20–50 percent of total production in all but one industry is not true. Moreover, Magdoff actually provides the data separately by exports and federal consumption so that it would be possible to deal directly with the former if that is at issue. Granted that a selected list of industries is used to begin with, the vast majority of them export 10 percent or less of their output (see table 42, p. 188). Moreover, no data are given on the countries receiving these exports. Since Western Europe, Canada, and Japan are the main export markets for the United States, is it the case that such a substantial level of military expenditures is necessary to protect such markets? Moreover, many of the twenty-four industries examined by Magdoff are ones which probably lose more from imports and domestic inflation than they gain by exports. At least if Stevenson is serious about the issues, these might well be considered in his future research on the topic.

When the chips are down, all I am advocating is that we try to do research on the empirical implications of this important issue in macrosociology. As pointed out in my paper, there are obviously a variety of ways in which military-industrial linkages work to the detriment of the society's general goals. It would be pretentious to claim that the issues considered in my paper will receive a definitive answer in any single research effort. But it is only through empirical research, not through the polemics best restricted to other roles, that sociology can be of value. The devices used by Stevenson in his letter may be desirable for debators, but in my opinion they contribute nothing to the problem at hand. And until we shift from slogans and cheap rhetorical tricks to the pursuit of knowledge, we will have nothing to say that is worth hearing.

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Book Reviews

The Pursuit of Loneliness: American Culture at the Breaking Point. By Philip Slater. Boston: Beacon Press, 1970. Pp. xii+154. $7.50.

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Philip Slater's new book argues a thesis about American culture which specifies and elaborates upon Freud's insight that the creation of culture is a form of substitute gratification. That form is particularly perverted in America, argues Slater, by the pervasiveness of a version of the Frankenstein myth: a runaway technological monster upon whose monstrous products most of the eroticism of Americans is focused. The sexualization of technology (things) is the mechanism by which the Original Abundance (sexual gratification) is transformed into a psychology of scarcity which has made Americans competitive, individualistic, and acquisitive, but rarely satisfied. In eagerly producing the frustrations they endure, Americans manufacture the repressed rage and violence which make the random killing in Vietnam possible. They imprison women in desexualized domestic roles. They promote generational conflict through child-rearing techniques that make parents either vampires or impressarios, and through the mutual denial by parents and children of each others' genuine sexuality. Finally, they systematically deprive themselves of their needs for community, engagement, and dependence; and these frustrations have spawned the communal and hedonistic "new" or "counter" culture which has pushed the old culture to the breaking point because there can be no genuinely moral accommodation between them.

That is the thesis. Is Slater right? The very question seems almost impertinent. This country is so fragmented into a multiplicity of milieux that by generalizing from any one or few of them one can plausibly argue just about anything one wishes—particularly if one discounts the relevance of attitude and other survey data. There are theories available among which we can pick and choose to characterize Americans as greedy and mean spirited or generous and warmhearted; compulsive and repressed or casual, relaxed, open, and easy going; rigid and demanding or flexible and tolerant; individualistic and competitive or herdlike and nostalgic for community. Add to these complexities the probability that each milieu is itself replete with built-in structural contradictions and the psychological ambivalences that usually accompany them, and it becomes an intellectual triumph of sorts to say anything at all which is important, general, and wholly true.

Philip Slater knows all this but the difficulty does not seem to inhibit him much. The lust (I use the word advisedly with respect to a Freudian book) by commentators on American Culture to say something vital about it is apparently limitless (like other substitute gratifications) and spills

over all obstacles to utterance. Slater's lust is indulged by exploiting a theory, by milking a metaphor, by selectively using and ignoring evidence, and by the literary device of using a "we" (it is not an editorial "we" nor a scientific "we" but more like a Royalist "we")—as in "We also feel bored and uneasy with the orderly chrome and porcelain vacuum of our lives" (p. 16) in order to seduce the reader into believing that the writer's personal impressions (projections often as not—as good insights usually are) are self-evident truths spontaneously arrived at and shared by both.

Because the theory is a fertile one and because Slater is a gifted and sensitive partisan of it, he is frequently brilliant—particularly on sexual matters, for example in his insight that packaging has become a major industry in America because buying things has become so sexualized that "carrying naked purchases down the street in broad daylight seems indecent" (p. 94). Or in his assertion that romantic love at first sight must be oedipal because, based upon nothing, it can only be transference at work.

He is very good on other things too. The insight that gives the book its title is that technology, by substituting machines for people, reduces the occasions for real interaction; hence, in affirming machines, Americans "seek" loneliness. The GNP, which symbolizes the preference of Americans for things rather than people as sources of gratification, "will reach its highest point when a material object can be interpolated between every itch and its scratch" (p. 93). One of his favorite metaphors is what Slater calls the "Toilet Assumption," by which he refers to the complacent reliance by Americans on the flush toilet to get rid of their shit and their other embarrassing effluent—so much so that when the problems which have been systematically evaded and avoided erupt into headlines (riots, etc.) "we react as if a sewer had backed up" (p. 15)—with shock, anger, and disgust. Slater is good, too, at using what Kenneth Burke called "perspective by incongruity" to overturn the homilies of Protestant industriousness: "One thing can be said for idleness: it keeps people from doing the Devil's work" (p. 138).

Such tidbits make the book very worthwhile reading. It is surely a better book than either Theodore Roszak's *The Making of a Counter Culture* or Charles Reich's *The Greening of America,* two books whose biases are similar to Slater's but which have received far more attention And yet Slater's book is undermined by what is probably the very motive that gave it birth. For one does not write a book like this unless one is repelled and disgusted by the quality of life in one's own country and has the prophet-redeemer's impulse to cleanse it "before it is too late." And for this purpose, exaggeration, distortion, and self-contradiction are not excessively high prices to pay.

Slater begins revealing his revulsion with America as early as the preface where, upon returning from abroad, he is struck by "the grim monotony of American facial expressions—hard, surly, and bitter—and by the aura of deprivation that informs them" (p. xii). Later on, discussing the war in Vietnam, he suggests that Americans enjoy killing defenseless people

because they themselves suffer "mass impersonal injuries from mechanical forces against which they, too, are powerless" (p. 42); and still later, commenting upon the peculiar sexual attitudes of censors toward nudity and violence, he flatly asserts that "the reason a mutilated body is more acceptable than a whole one is that it is only in mutilated form that the sexual impulse can exist in America" (p. 92). Slater does not bother at all with the evidence for or against such powerful statements, although he rejects the possibility that guilt-induced impotence and frigidity may be on the increase (which would weaken his hopes for the new culture) on the grounds that "there are no sound data on which such a statement can be based" (p. 79). The absence of "sound data," in short, prevents his believing only what he prefers not to believe. But in a sense the evidence *is* irrelevant because many of Slater's assertions are significant, not for what they reveal about Americans, but for what they reveal about his moral feeling and about the intensities which sustain him as a humanistic intellectual.

The author's antipathy to the old culture and his sympathies for the new lead him into rhetorical excesses that ultimately blunt even his sensitivities as a sociologist. "What," he asks, a propos the hostility of straights to hippies, "is so severely lacking in our society that the assertion of an alternative life style throws so many Americans into panic and rage?" (p. 5). The question is rhetorical, because to him the answer seems self-evident: the straights suffer deprivation while the hippies live pleasurably. But the new culture, as Slater himself admits, *is* a genuine threat to the old, and surely one does not need a Freudian hypothesis to explain a hostile response to a genuine threat to one's values and style of life "How," Slater asks with surprising sociological innocence, "can matters so intrinsically trivial as hair length or apparel arouse reactions of such intensity in people who present themselves as the most sane, stable, and effective members of our society?" (p. 61). Well, because they are not trivial matters. One might as well ask how some stars and stripes on cloth or two crossed sticks can arouse such intense response. Slater's antipathy takes him even to the assertion that "the old culture is difficult to explain without recourse to psychopathology" (p. 120)—apparently without realizing that this is exactly the tactic of the old culture in putting down the new (hippies and other deviants are sick sick sick). Nor does he hesitate to contradict himself; so when it suits his purpose he can characterize middle-class bureaucratic life as a "treadmill," but when talking about the grievances of women he can turn right around and say that what women want are the very things that men get from their middle-class jobs: meaning, stimulation, challenge, social satisfaction.

Still, I have the gnawing suspicion that my mixed response to this book simply does not matter much. It is the sort of book written for those who already agree or almost agree with its plea for a social radicalism combined with a technological conservatism (a reversal of the American pattern) and for those who might join the author in mounting a twin assault on the institutions of repressed sexuality and on the psychology of scarcity which

sustains them. They will say "Right On!"; Middle Americans will see only another academic America hater. And I futilely try to stay detached about a book meant precisely to engage the reader. I think often that silence—or near silence, anyway—might be a better alternative; in these days of verbal overkill, terse remarks just may be the highest form of expression.

Engagierter Expressionismus: Politik und Literatur zwischen Weltkrieg und Weimarer Republik. By Eva Kolinsky. Stuttgart: J. B. Metzlersche Verlagsbuchhandlung, 1970. Pp. vii+232.

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German expressionism, one of the earliest artistic manifestations of the revolutionary upheavals of the twentieth century, began as a revolt against existing art forms just before World War I. But it soon broadened into a revolt against modern civilization and against bourgeois life-styles, and culminated in a full-scale attack against the whole social order.

The turbulence, stridency, and highly charged imagery of expressionistic writing reflected its passionate rejection of things as they were. Screams, visions, apocalyptic evocations replaced the tame rhythms or the naturalistic descriptions of previous literature. "The poet," wrote the expressionistic poet Johannes R. Becher, "beats the drums shrilly. He stirs the people with staccato sentences." Major expressionistic reviews had titles such as *Action* and *Storm*.

Eva Kolinsky's study is mainly devoted to a detailed analysis of contributions to these expressionistic periodicals during the war and immediately after. She shows that the pacificism or revolutionary socialism of most of these reviews tended to be highly abstract, Utopian, and insufficiently rooted in concrete analysis. Celebration of generic Man, of Fraternity, Community, Love, stand side by side with rejection of "soulless" machine civilization and "materialistic" technology. Lyrical celebrations of the brotherhood of all men went hand in hand with exaltation of the artist as the chosen vessel of the ideal order to come.

During the war, those expressionistic reviews which managed to survive the censorship indulged in passionate denunciations of the massacre and appealed to human solidarity against the fratricidal slaughter in the trenches. When the revolution of 1918 put an end to the war, these periodicals expressed disappointment with the social-democratic regime which had murdered Rosa Luxemburg and Kark Liebknecht, put down the Spartacist uprising of the extreme left, and inaugurated a return to humdrum normalcy. Some exalted the Russian example and called for its emulation. Others extolled a vaguely humanistic and nonviolent socialism, and others again gradually withdrew all affect from the public scene advocating the cultivation of private sensibilities or extolling the superior virtues of youth or of the artist.

The attempt to fuse political activity and artistic creation proved unsuccessful. Political engagement and praise of the common man gradually receded and gave pride of place to the traditional German glorification of *Geist* (spirit) and its predestined defenders, the artist and writer. Spurned by history, disappointed by its failure to eventuate in Utopia, most expressionistic writers returned to inwardness. Some of them, like Becher and Brecht, later made common cause with the Communists. Others, like Benn and Bronnen, became Nazis. Most of them, however, gave up the attempt to fuse the spheres of art and politics, of literature and revolution.

Kolinsky gives many concrete illustrations of the failure of expressionistic intellectuals to come to terms with the political realm. Neither Schickele's hope for a revolution to be brought about through the redeeming power of the heart, nor Leonard Frank's fervent belief in the goodness of man, provided sufficient leverage to come to grips with the recalcitrance of political events. There is something pathetic, as well as amusing, about the proclamation of the Berlin Political Council of Mental Workers which demanded, shortly after the revolution, "the abolition of academies, socialization of all theaters, socialization of all free professions, abolition of all titles, and the immediate convocation of a world parliament." The expressionists were hardly equipped for effective intervention in political affairs. They were marginal intellectuals, far removed from everyday affairs, who cultivated a wholly exaggerated sense of their own importance.

This book will be of help mainly to those who are already familiar with the historical landscape of twentieth-century Germany. It does not provide the social and historical background. It is of interest to those concerned with the sociology of men of ideas and with the complicated ways that link artistic productions to their social matrix.

Political Conflict: Essays in Political Sociology. By Morris Janowitz. Chicago: Quadrangle Books, 1970. Pp. xii+271. $8.95 (cloth); $2.95 (paper).

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These really *are* essays in political sociology. They are written as essays rather than working papers, and the subject is plainly and unabashedly political sociology. There is nothing precious or self-conscious here. No exquisite but tedious delineations of what a "sociological" treatment of political behavior, history of concepts, theory, or the comic pages would have to consist of. No prescription at all, no theology, no theory of theory of theory. Just some hard-headed, sophisticated, and alert reactions to what is going on below the surface of life in the twentieth century. And, if anyone is still interested, the book is relevant; to the sociological past, to present outlines and tendencies of the discipline, and, mostly, to major processes at work in society.

The essays are nicely divided into sections: The Institutional Framework, Social Structure and Political Affiliation, Macrosociology and Armed Conflict, Political Violence in American Institutions, Methodological Dimensions, and, finally, Sociological Analysis and Policy. Essays within these sections range from the thematic and conceptual "The Logic of Political Sociology," a very useful review of styles from Tocqueville down to the present, saved from narrative tedium by its emphasis on the styles, to such splendid topics as "Patterns of Collective Violence," "Native Fascism in the 1930's," and "Military Elites and the Study of War." In between are pieces on mass communications, relations of civil and military powers in the new nations, community power in this country.

Clearly, the range is wide, subject matter diverse, evidence of hard work great. I could wish more were to be found on the military, for Janowitz is without question our preeminent interpreter of the military establishment. Not since Tocqueville's fascinating chapters on the military in the second volume of his *Democracy in America,* where the military is dealt with, as it should be, as a social system, has anyone approached this immense sphere of modern life as Janowitz has. In his "Military Elites and the Study of War" in this volume Janowitz has some extraordinarily perceptive observations to make, not merely on the study of the military, but on the relation of the military to modern civil life. Among them is his (to me) utterly convincing demonstration that by trying to "civilianize" the military one is certain to wind up with an organization that is neither civil nor military and in which new forms of hostility and unanticipated militarism could run rampant. (Future McNamaras and Pentagon Whiz Kids please take note!)

Janowitz's "Civil-Military Relations in the New Nations" throws much light on forces of conflict and, distinguishably, on forces of change in these societies. I particularly like his criticism, restrained but direct and effective, of the underlying theme of progressive developmentalism in so many of the accounts sociologists and political scientists—especially the latter—have given us of the transition from "traditional" to "modern." Such a theme is, in Janowitz's phrasing, "a continuation of the 'idea of progress' in a non-Western context." Of course if one separated out this theme from the vast majority of studies of "new nations" and so-called development, there would not be much left but heaps of data. But so much the worse for the studies.

"Native Fascism in the 1930's" is at once a fine evocation of such movements as the Christian Front, the Black Legion, Father Coughlin's radio audience, all active a generation ago, and, more subtly but clearly, a warning of the form such movements would take, will take, or are taking, possibly, in the last third of the twentieth century. "It does not seem likely that fascism could be the effort of a small handful of men seeking to cope with the economic and strategic insecurities of America in an authoritarian fashion. It requires a cadre of skillful organizers and propagandists to lead wide sections of the population to reject our democratic institutions." Janowitz quotes Huey Long's celebrated observation that

if fascism ever comes to America it will be called Americanism. But as long as we are in the realm of speculation here it might be doubted that today "Americanism" would suffice. We have become a more educated and sophisticated nation, at least in the burgeoning middle class. "Common Cause" would make a far more attractive label today, I think, and would have the advantage of implicit insights drawn from sociology of what the real roots and contexts of fascism are. It would be free of the gaucheness of "Americanism" and of unpleasant Communist and Nazi associations with the once-hallowed word "people."

I hope no one will miss, if he has not already read, the final essay in the volume, "The Ideology of Professional Psychologists." The theme of Janowitz's remarks extends beyond the psychologists, taking in, for sure, sociologists. The warning is friendly and relaxed, but not to be missed: Let us put an end to self-contemplation by professions and disciplines of their true identities. Such navel-gazing will get us nowhere. As Janowitz writes—and his words these days could have been as easily and justly directed to sociologists—"The major intellectual spokesmen for recent American psychology have not been professional psychologists: Veblen was an economist, Dewey a philosopher, and, more recently, Kinsey a zoologist, and Riesman a lawyer turned sociologist." (The essay, it should be noted, was written in 1954, and appeared in the *American Psychologist*.)

Giving fitting end to both the essay and the volume is Professor Janowitz's reminder that despite fashionable belief to the contrary (I gather there was a New Left back in 1954; let us call it the apostolic New Left), there is no evidence to support the view that social scientists as such are unwitting masters of "human exploitation and manipulation." All one needs to do, Janowitz suggests, is observe the fate of social science and of social scientists under totalitarian systems. "I am convinced that the efforts of social scientists, if they are constantly mindful of the inherent limitations and biases in scientific knowledge, will serve to enhance the dignity of mankind by clarifying the fundamental nature of human nature." As I say, the words were written nearly twenty years ago. How prophetic can you be?

This is a splendid book in itself, as well as a fine emblem of the larger scholarship from which it is drawn by one of our outstanding social scientists. And its unfailing lucidity of style puts it in reach of everyone—professional, student, intelligent general reader.

The Social Contract: A Personal Inquiry into the Evolutionary Sources of Order and Disorder. By Robert Ardrey. Drawings by Berdine Ardrey. New York: Atheneum Publishers, 1970. Pp. 405. $10.00.

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In 1936, Brooks Atkinson reviewed *Star Spangled*, the work of a new

playwright, and declared, "Robert Ardrey has very little skill in expressing his ideas in play form." In 1962, Atkinson devoted one of his "Critic at Large" columns in the *New York Times* to Ardrey's first book of nonfiction, *African Genesis,* and noted Ardrey's contempt "of Rousseau's thesis that men are born innocent and corrupted by society." Atkinson summarized Ardrey's key idea in the phrase, "we are born of risen apes, not fallen angels." Now, less than ten years after, Ardrey returns to this contest with Rousseau, deliberately flaunting the title of one of Jean-Jacques's most famous essays and with sufficient *chutzpah* to dedicate the whole barrel of tripe to the memory of the master. Of course, Ardrey's concept of the social contract differs from that of the Enlightenment, as his nineteenth-century outlook contrasts with that of the eighteenth. As put by Ardrey, "There is no more severe distinction between my social contract and his than that Rousseau's was an agreement between fallen angels, while mine is one between risen apes" (p. 101).

Taken as a persona, Ardrey is more interesting than his literary corpus. Anyone reading his latter-day version, which takes subtitular advantage of present-day concern about violence and disorder, has some basis for concluding that Ardrey is a racist; a sexist; a hater of the young; a cynical believer in ineradicable, innate human aggressiveness and instinctive territoriality. Such a reader might think that Ardrey's was a simple, brutal, stupid mind. How else to interpret his argument that "the Negro" enjoys superior endowment for athletic performance, but is genetically poorly equipped for the classroom (pp. 56–65)? The level of Ardrey's current sensitivity to racial problems is revealed in offhand comments, as in this example: "One recalling Darwin's cattle may turn to more recent naïvetés such as the expectation that black Africa, in our view an enormous undifferentiated herd of dark-skinned people, would willingly unite for rational ends" (p. 68). Indeed, there is this capsule evaluation of the recent history of the black struggle: "For all the Negro's profound and unarguable grievances, there has not been a racial outbreak in America since the days of Watts in which a degree of carnival atmosphere has not prevailed" (p. 286). Of course, Ardrey's stereotyping is not confined to blacks. His knack for the trenchant phrase lightly blesses others: "The samango monkey is arboreal, brown and rather large, with the creased, lamenting face of some ancient Jew" (p. 75).

I think we may be sure that there is nothing personal in all of this. Ardrey goes out of his way to decry racial stereotyping at the instant he purveys it, and undoubtedly some of his best friends are you know what. Actually, Ardrey's venom is deliberately aimed at academic structure; it is clear that he does not like many of his previous reviews and particularly resents being treated as the amateur he is, rather than the scientist he fancies himself to be. His special targets are clearly staked out: "That the three sciences central to human understanding—psychology, anthropology and sociology—successfully and continually lie to themselves, lie to each other, and lie to the public at large, must constitute a paramount

wonder of a scientific century. Were their condition generally known, they would be classified as public drunks" (p. 12). The metaphor of drunkenness applied to sociology, anthropology, and psychology is very appealing to Ardrey; he uses it more than once. I presume that he considers scholars in these disciplines to be intoxicated with power, a common fantasy in certain sectors of the current scene.

The message of *The Social Contract,* drawn out for more than 350 purple-prosed, repetitive pages, is telegraphed in the foregoing. The drive for egalitarianism will send us over the precipice into unending night. The sensible alternative is to concentrate equality in the context of opportunity, thereby achieving a healthy, creative elite. It sounds like Ayn Rand or Andrew Carnegie. As remarked earlier, do not expect anything new here. But if the message is boring, the medium is frenetic; I would entitle this book *La Gazza Ladra* [The thieving magpie]. Every glittering bit of ethological business has been collected and brought back to Ardrey's nest, where jewels are mixed with *drek.*

Ardrey has had this problem. Reviewers have complained about his technical abilities since the 1930s, when, after an unreadable "novel about Magdalenian man," and several equally unreadable and unplayable plays, Ardrey caught Jed Harris's eye or ear and had his first turkey produced, the previously mentioned *Star Spangled.* Even the *Literary Digest* (March 21, 1936, p. 19) turned that one down. Several later plays seem to have been worse.

Although some continuities of thought and theme mark Ardrey's work throughout his career, my impression is one of discontinuous trajectory. I am not referring to his sojourns in Hollywood and the contrast between the writing he did for the dream factories and that he offered the stage. The main break occurs a little later, when he went off to Africa as a correspondent for the *Reporter* magazine. There Ardrey met and interviewed Dr. Raymond Dart, the first discoverer of the Australopithecines. From this moment Ardrey's life seems to have changed.

Pre-Dart Ardrey was a man who liked controversy and used drama and the novel to project his feelings and views. He was associated with such progressive ensembles as Group Theatre and the original Phoenix Playhouse Company. His themes included racial injustice (*Jeb,* 1946) and McCarthy-type political persecution in the United States (*Sing Me No Lullaby,* 1954).

Except for one script, *Thunder Rock,* Ardrey seems never to have enjoyed an audience success. Even that play flopped in the United States, although it had a long run in England where it was finally made into a film. Otherwise, in the United States, Ardrey's career seems grim. Some of his plays closed almost immediately. *Jeb,* mentioned above, lasted nine performances. Lewis Nichols, the *New York Times* critic, had flattered the idea of the play, writing that "because of its substance and honesty, it is the type of play which has an audience always on its side." However, Nichols sadly concluded that, as a job of playwriting, *Jeb* "does not hold

up its share." When Nichols discussed the folding of *Jeb* in a later column, Herman Shumlin, its producer, fired in an angry letter attributing the failure to hostile critics (*New York Times*, March 3, 1946, p. 2, col. 2).

Did the unkindnesses of critics and the neglect of audiences have anything to do with forming Ardrey's present dark view of most of the human race? He does not seem to think so. In a recent view of himself he projects back into his youth an image that fits neatly his post-Dart work. Describing his own ruthlessness in fighting his way into a Thorton Wilder writing seminar at the University of Chicago in 1930 (when he was 22), Ardrey says, "I make no apologies, but I have an inexplicable gift for survival under conditions of jungle warfare" (*Plays of Three Decades*, 1968, p. 10). Ardrey has been in jungle combat only metaphorically, at war with critics and audiences.

Geoffrey Gorer, an anthropologist, has paid tribute to Ardrey (in *Encounter*, 1967, p. 76), declaring that despite his shaky reasoning and conclusions, Ardrey's "facts" can be relied upon. I am less sanguine about this. The magpie quality of Ardrey's assembling of evidence has already been alluded to. One anecdote gives way to another, sometimes documented, sometimes hearsay, sometimes out of his own casual experience. It is very difficult to check up on such saltations. However, apart from plain nastiness, I have caught some of his errors or distortions. Two will have to do as examples. One arises from Ardrey's use of his favorite materials, which come from highly skilled naturalists specializing in the ethology of one or a restricted number of species. George Schaller is one such scientist who is held in highest esteem by Ardrey. Yet, I think that Ardrey has been unhappy with some of Schaller's work on the gorilla because of its indication that gorilla groups are relatively open. At the same time, the Reynolds have given us a clear view of the open nature of chimpanzee bands. This sort of material threatens the Ardrey thesis of the antagonistic, tightly territorial, prehuman past. Whatever the motive, however, Ardrey is distorting when he assures his reader that, "gorilla society cannot be described as open" (p. 274).

Another serious lapse occurs in Ardrey's use of Robert F. Murphy's work on the Tuareg. (Ardrey refers throughout to the "Tauregs.") According to Ardrey, Murphy says that "the veil before the face protects the personal distance of the alpha Taureg [sic] precisely as dark glasses protect the space of the western alpha. I shall watch you: you will not watch me" (p. 229). But Murphy actually contradicts Ardrey at several critical points. Thus, particularly in encounters between ordinary Tuareg and their chiefs, "the person of higher status will usually keep his veil at a somewhat lower level." Indeed, the most prestigious persons in the society, the *hajji*, simply do not wear veils at all. It is clear that the main use of the veil is in the context of relations among affinals. So much for Ardrey's accuracy.

If this review has been more *ad hominem* than *ad librum* it is only because the book is really not worth serious consideration. This, in turn,

raises questions about the sociology of publishing. Why do so many books like this get such keen attention from publishers and the mass media, when much better books on similar topics escape virtually without notice? I guess that is really not such a hard question.

A Sociology of the Absurd. By Stanford M. Lyman and Marvin B. Scott. New York: Appleton-Century-Crofts, Inc., 1970. Pp. x+221. $3.50 (paper).

Social Psychology through Symbolic Interaction. Edited by Gregory P. Stone and Harvey A. Farberman. Waltham, Mass.: Ginn Blaisdell, 1970. Pp. 738. $11.95.

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The broader significance of these two books is related to current dissatisfaction with, and demands for alternatives to, functionalist modes of sociological analysis. Each attends to this task in a different way.

A Sociology of the Absurd is a collection of seven articles (four of which are appearing for the first time) by Stanford Lyman and Marvin Scott. Each piece embodies one or more aspects of the theme of "absurdity," which prefaces and organizes the contents of the book.

Absurdity is a perspective that is grounded in certain "background assumptions" about the nature of man and society. The first is the existential-phenomenological idea that social life admits of no inherent meaning, that reality is negotiated rather than given. Interaction, accordingly, is mediated by interpretational or definitional (i.e., symbolic) processes. Such processes are examined in the analyses of time ("On the Time Track") and space ("Territoriality: A Neglected Sociological Dimension"). The authors suggest that the sociological importance of these two dimensions resides in the temporal and spatial referents of the self. Identity is shown to be, in part, rooted in the sociotemporal/spatial order which is, in turn, the embodiment of competing claims, meanings, conventional divisions, and the like.

One implication of the Lyman-Scott version of subjective realism is the importance of the engineering of meaning in social relations. This emphasis is most conspicuous in "Accounts," which treats the manner in which seemingly untoward behavior is rationalized or coated with conventionally acceptable intent. The boundary between conventionality and deviance is thereby negotiated.

A second assumption of the sociology of the absurd is that, because the ends to which human action is oriented are scarce, conflict is inherent in social relations. According to Lyman and Scott, the conceptual framework that is most compatible with this assumption (in the study of face-

to-face interaction, at least) is the game model. In "Game Frameworks," an attempt is made to formulate, in the symbolic interactionist mode, a typology of games, while "Paranoia, Homosexuality, and Game Theory" considers the core feature of the game, namely, the Meadian phenomenon of the self taking itself as an object from the standpoint of the other. The pathological possibilities inherent in this process were ignored by Mead but are herein depicted.

To the extent that their outcome is both consequential and problematic, games constitute a field for the display of "character." The sociology of this form, which refers to the capacity to conduct oneself effectively under stress, is dealt with in "Coolness in Everyday Life"; the contingency of its default, in "Stage Fright and the Problem of Identity."

Unfortunately, the reader will not find *A Sociology of the Absurd* to be organized as tightly as this review implies. The book would be far more valuable if each article were preceded by an editorial treatment of that particular aspect of absurdity to which it refers. As it stands, the theme of absurdity is insufficiently related to the several worthwhile articles which it is meant to organize.

In *Social Psychology through Symbolic Interaction*, Gregory Stone and Harvey Farberman attempt to clarify symbolic interactionist principles and to display their application. To this end, sixty-nine articles and extracts are presented; these are divided into ten parts, each with an editorial preface on the universe of theoretical and empirical content which the articles sample. The coherence with which these universes are defined and the representativeness of sampling varies from one part to the next.

Symbolic interactionist principles receive their sharpest exposition and illustration in the sections on deviance, definition of the situation, and motivation. The first-mentioned section deals with the interactional grounding of deviance, with the focus on labeling theory, while the last conveys the unique perspective of motive as a socially conditioned rationalization of acts. The readings on the definition of the situation make concrete the postulate of subjective realism and the problematics of managing such definitions. In the latter respect, however, the omission of Goffman in favor of those inspired by him is puzzling.

The least adequate parts of the book include the section on interaction process. Major omissions here are the ethnomethodological, dramaturgical and game-theoretical perspectives and, most important—and this is a major failing of the book—the monumental work by Goffman on the properties, structure, and dynamics of face-to-face interaction.

The section titled "Tests of the Perspective" is inappropriately labeled and its content misplaced. A perspective is not subject to verification; it is useful or not, depending on its capacity to generate ideas which are. Furthermore, no separate section on techniques seems to me to be warranted because none are presupposed or ruled out by the symbolic interactionist perspective. The articles in this section, centering as they do on identity, therefore belong in the one on the self.

Unfortunately, one of the weakest points in the book is the introduction, which sets for itself the goal of isolating the definitive characteristics of the symbolic interactionist framework. The basic content involves a couple of polemics against functionalism (the best of which is directed from the psychoanalytic and not the symbolic interactionist standpoint). The problem here is that its inconsistency with functionalism makes obvious neither what symbolic interactionism is nor what is unique about it.

Other sections of the book, namely, situations and social worlds, the self, socialization, and symbolic mediation of interaction, seems to be fairly adequate in respect to content and organization. The book ends puzzlingly with a long appendix on the effects of psychotherapy!

Taken as a whole, however, this collection, except for the very important omissions noted, will provide the student with a basic view of what the symbolic interactionist perspective brings to the relationship between men and groups. The book's quality, though, does not match its considerable quantity, wherein resides its chief value.

Although Stone and Farberman were bothered by psychoanalysis, Lyman and Scott were able to ignore it. Functionalism, however, serves as a foil common to both books. Both collections, apparently, were consciously put together with a view to providing an alternative to the functionalist image of man as a passive object molded by the group. Both books, in some measure, embody such an alternative. Yet, by so dismissing functionalism, the editors skirt the problem of its relation to their own positions. For example, by failing to treat systemic as well as individual referents of meaning, along with their convergences and discrepancies (as did Merton in his analysis of manifest and latent functions), their own views are unnecessarily narrowed. Similarly, by their polemical stance toward psychoanalysis, Stone and Farberman neglect its rather exciting parallels with symbolic interactionism. Just as the self presented in their readings is an active, negotiated, and negotiating agent, so the development of ego psychology, initiated by Freud himself in the late twenties and early thirties, emphasizes the autonomy of the ego and its negotiating capacity.

By stressing what is negotiated in social life at the expense of what is not, neosymbolic interactionism seems to be leading us from (an originally Durkheimian) oversocialized conception of man to an undersocialized conception. Current overreactions to functionalism, among which these books might in many respects be numbered, could therefore profitably be abandoned in favor of a more informing confrontation. Contemporary ego psychology, particularly the Eriksonian mode, might be enlightening in this respect, for while it addresses the active functions of the ego it does not (paradoxically) deemphasize, as much as its neosymbolic interactionist counterparts, the superego and the normative demands that it mediates. It is significant, in fact, that the sociology of the absurd takes as its founder Machiavelli rather than Mead, who was more sensitive to the

Durkheimian insight that while men negotiate, plot, and even defraud, they do so under very powerful constraints.

Utility and Choice in Social Interaction. By Lynne and Richard Ofshe. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. ix+202. $7.50.

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This is an important book for at least two reasons. First, it reports on an ingenious application to social situations of a theory first developed to explain individual behavior in light-guessing experiments. Second, the theory is applicable to highly diverse social situations and to the analysis of attitudes and motivation as well.

Despite recent disenchantment with game theory, Lynn and Richard Ofshe are convinced that decision making and game theory can be used to predict social behavior. They start by distinguishing between two processes that take place in coalition games, choosing partners and bargaining over rewards, but focus theoretically on the explanation of choice behavior. Choosing partners is viewed as a decision process in which each player maximizes his expected utility. It is assumed that expected utility has three components: (*a*) the utility of the reward received from a winning coalition; (*b*) the probability of obtaining the reward associated with each alternative coalition; (*c*) the utility for equity in allocating the total available rewards among players. In order to predict how the three components combine to affect coalition choices, the Ofshes adopted S. Siegel's formal model of the way individuals guess which of two light bulbs will light up on each of a series of trials in which the probability of any light coming on is predetermined by the experimenter. In adopting Siegel's model, the Ofshes show that the utilities in coalitions are perfectly analogous to those in the light-guessing experiment. In addition, they adopt Siegel's assumption that the three components combine additively.

Testing of the theory was carried out in three stages. First, a series of experiments were conducted to see if each of the three independent variables had the anticipated causal effect on coalition choices. Second, a series of experiments were conducted to see if the model would accurately predict choice behavior when one or more of the independent variables were changed. In these first two series of experiments, single human subjects played coalition games with two robot partners simulated by computer. By using robot partners, control was established over the probabilities of each human player being chosen. The expected utilities for correct choices were varied by changing the monetary values of coalition payoffs. Expected utilities for equity were manipulated in pregame in-

structions by differentially stressing individual gains and losses, by differentially stressing the interdependence of players, and by differentially leading the subjects to believe they would meet their game partners. A third series of experiments, in which all three players were human, was carried out in order to see whether the model would accurately predict behavior in freely interactive situations. Finally, the relevance of the theory for other interactive systems was demonstrated in analyses of two-person conflicts (prisoners' dilemma) and N-person systems, such as simple chains and networks.

While the choice model must be supplemented eventually by a model for bargaining, it is a significant contribution to the work begun by von Neumann and Morgenstern. There is no reason why the theory should not now be tested in more realistic situations. For example, the model offers a solution to the Condorcet paradox. The paradox is embodied in a situation where three persons or groups have the preferences for x, y, and z alternatives shown below and must decide upon a single alternative. As

Group or Person	Preference		
	1	2	3
A	x	y	z
B	y	z	x
C	z	x	y

it is usually conceived by political theorists, no solution to the paradox is compatible with the conditions of political equality and popular sovereignty. However, if it is assumed that such situations are recurrent and that coalitions are permissible, the Ofshe's model should predict how such paradoxes are resolved—as of course they are in the "real world."

The three independent variables in the model appear to be directly analogous to those used by J. W. Atkinson, Victor Vroom, and others to explain motivation, and by Helen Peak and others to explain attitudes. Vroom, for example, theorizes that aroused motivation is a function of the importance of a particular value, the relevance of the consequences of acts for attaining that value, and the likelihood that the performance of an act has certain consequences. The main difference between the two theories is that the effects of the independent variables are assumed to be multiplicative in Vroom's model, whereas the Ofshes assume that the effects are additive. Therefore, a "crucial experiment" seems warranted in order to test the relative superiority of the two theories.

In summary, the theory is immensely powerful in its capacity to precisely predict behavior in a wide variety of social situations, and appears to be potentially applicable to the study of motivation and attitudes. Yet a *caveat* is in order, for what the Ofshes treat as *in*dependent variables

most sociologists treat as *de*pendent variables. Thus, while it was shown that the utility of equity is constant even when other characteristics of the game are changed, this constancy was achieved by not letting players communicate with each other. In real life players *do* communicate, establish definitions of the situation, and thereby affect each others' utilities for equity. In addition, payoffs were guaranteed after coalition formation and, as noted above, the utility of equity was experimentally manipulated in pregame instructions. Again, in real life, this power of the experimenter to manipulate game rules and to enforce the "contracts" made in coalitions is typically vested in institutionalized roles such as legislator, judge, referee, umpire, jury, etc. Perhaps the Ofshes are correct in placing greater priority on achieving precision of prediction than on researching the substantive problems implied by the institutionalization of such roles. But I suspect that sociologists would be wiser to ask why various situations are defined differently, who can and should have the power to manipulate and enforce the rules of games, and under what conditions solidarity develops so as to affect players' utility for equity.

Topics in Regression Analysis. By Arthur S. Goldberger. New York: Macmillan Co., 1968. Pp. xii+139.

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The title of this book suggests that it is essentially a series of essays, each more or less self-contained. However, the book (which is based on eight lectures given by Goldberger in 1965) might better be described as an overview of regression analysis from an unorthodox perspective. The style is relatively informal; although there are many proofs given, they generally are elementary. Topics are treated in an outline form, but the coverage is fairly comprehensive. The book includes discussions of the interpretation of R^2s, of measures of precision of regression coefficients, and of the derivation of the classical properties of least-squares estimators in the random X's case. Also discussed are ways to transform variables to obtain linearity in the regression, and considerations in choosing a particular functional form for the regression model in an actual application.

In the preface, Goldberger states that his objective is to discuss "a selection of topics from the point of view of a researcher who is interested in using this important tool in a productive way in empirical investigations" (p. vii). This statement is misleading, for the book contains no detailed discussions of actual applications of regression analysis to data, though there are some passing references. Throughout most of the book, the reader will look in vain for practical guidance as to good and bad practice in the use of regression techniques. Only in the last chapter is an effort

made to give practical advice. And that advice, while quite sensible, is hardly of the sort to be engraved over one's desk. In short, then, this book is not at all what its title and preface might lead a social researcher to expect.

It is something quite different, and in many ways quite interesting. The major part of the book (chaps. 2–6, and partially 7) consists, as it were, of a guided tour of the whole subject of regression analysis, undertaken with a somewhat selective perspective, and with an occasionally elevated level of difficulty. What makes this effort interesting, in comparison with others already available, is that it utilizes several unconventional ideas to explain the nature and features of regression analysis. Thus, this book is likely to be of most interest to someone who has already learned a good deal about regression analysis from conventional sources, but who wishes to deepen his understanding of it. It is more nearly an hors d'oeuvre for the budding expert of regression analysis than a bit of lean meat for the hungry data analyst.

Goldberger's unconventional perspective can be illustrated by three examples. At the beginning of the book, he describes regression analysis in terms of conditional expectations That is, he tells us that regression analysis is a particular way to analyze stochastic relationships. A stochastic relationship is one in which the conditional probability distribution of one random variable differs for different values of another random variable. The distinctive feature of regression analysis is that, in order to make the analysis more manageable, it simplifies the consideration of these relationships so that only the conditional expectation is analyzed, and not the entire conditional distribution. This point is hardly novel, but it is one which seldom is presented clearly in books accessible to a general audience.

A second example of Goldberger's novelty is his approach to estimation procedures in terms of what he calls the analogy principle of statistical estimation. By this he means that if one wishes to estimate a population parameter (e.g., the expected value), he usually can use the statistic which has analogous properties in the sample (e g., the sample mean). In particular, if one wishes to estimate a set of regression coefficients for a population, the analogy principle can be applied by recalling a characteristic property of regression disturbances in the population. In the population, the disturbance variables have zero expectation and are completely uncorrelated with any of the regressor variables. Using the analogy principle, Goldberger suggests that we choose an estimating method which will preserve the analogous property in the sample. That is, if we let e_t denote the calculated residuals (which are to be distinguished from the population disturbances) from the estimated regression line, then we wish to insure that, for a sample of size T,

$$\sum_{t=1}^{T} X_{kt} e_t = 0$$

and

$$\sum_{t=1}^{T} e_t = 0.$$

By making this assumption (which he justifies only by appeal to the analogy principle), and then substituting for e_t its equivalent in terms of Y_t, the X_{kt}'s, and the estimated coefficients, Goldberger derives a set of simultaneous equations which can be solved for the b_k, which are the sample estimates of the population regression coefficients. These estimating equations are, it turns out, identical with the normal equations which appear in conventional regression analysis, and thus the solutions also are identical. This derivation can be accomplished without any use of calculus, which may be of interest to some readers.

A third unconventional idea used by Goldberger is his measure of the precision of an estimated regression coefficient. He defines this precision in terms of the size of the change in a regression coefficient needed to create an increase of 1.0 in the error sum of squares. The smaller that number is, the more precisely estimated the regression coefficient is. Goldberger calls this the "index error" of the regression coefficient. A related definition, introduced later, standardizes for the magnitude of the error sum of squares. Goldberger calls this latter the "radex" error of the regression coefficient. Finally, in chapter 7, Goldberger spells out the relationship between these indices and the more conventional measure of precision, the standard error of the regression coefficient.

For all three of these ideas, there is no fault in the logic of Goldberger's argument. What might be questioned is the generality of appeal of this particular intuition. Goldberger presents these approaches not as being superior, but as being unorthodox and perhaps intuitively appealing. For me personally, the approaches are interesting, but not noticeably preferable to the ordinary explanations. One criticism might be that all of these ideas are claimed to be of value to a relatively naïve audience whose grasp of the basic idea of regression needs strengthening. Despite that claim, the level of some of the arguments, and particularly some proofs which make use of Lagrange multipliers, is not at all elementary. In short, there is a real ambiguity about the intended audience. Moreover, for a reader whose understanding of regression analysis is incomplete, it would easily be possible to become confused rather than enlightened by reading this book.

The book should make interesting reading for a person who has some acquaintance with mathematics generally, plus a fairly good background in regression analysis. For the majority of its potential readers among sociologists, it can serve best as a supplement to some other treatments of regression analysis now available—including Johnston's *Econometric Methods*, Draper and Smith's *Applied Regression Analysis*, and Goldberger's own *Econometric Theory*.

The Logic of Images in International Relations. By Robert Jervis. Princeton, N.J.: Princeton University Press, 1970. Pp xi+281. $8.50.

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This book is a taxonomy of signals and indices, illustrated from the field of international relations, and an analysis of their operation as techniques for the projection of images. The author hesitates to call his image approach a paradigm (p. 16), but it does serve at least one function of paradigms: it shows that "previous explanations are inadequate and . . . [it provides] parismonious and satisfying accounts of a wide variety of events" (p. 16). The author is very modest about the limitations of the image approach: images constitute only one of the many influences on the predictions other states make about an opponent; images are sometimes ignored, sometimes there is little a state can do to modify its image; states are not unitary entities, so that domestic audiences complicate the projection of images for foreign consumption; and sometimes existing images of an opposing state distort the interpretation of any behavior not reconcilable with them. Having thus recognized the limitations of the image approach, Jervis assumes they are not operating in order to simplify his major purpose, namely, to show "how images can be cheaply projected" (p. 15).

"Signals are statements or actions the meanings of which are established by tacit or explicit understandings among the actors" (p. 18), intended to influence an opponent's image of the sender. Everyone is supposed to know what they mean. Their credibility, of course, is another matter; that can be established only later. So signals are essentially "promissory notes." We will prove it if you make us. Indices, on the other hand, are inherently credible. Part of their credibility lies in the fact that they are usually not under the control of the sender, although they may be manipulated by undermining the observer's assumptions about the motives of the sender (p. 43). In international relations hardly any indices are manipulable, a fact that makes it difficult to "project images on the cheap" (p. 65).

Signals are more aboveboard in the sense that they are meant to be read, but they can be used to convey inaccurate messages as well as accurate ones. Skill in using signals lies in the ability to know when to tell the truth and when to lie. If signals were always deceptive or acceptable only when shored up by indices, they could be ignored. But there are moral restraints on lying: there are important stakes in maintaining a complex system which is jeopardized by dishonesty in any part of it. Lies have consequences in changing the international environment, thus forcing behavior not initially contemplated. In democratic states, the public may not tolerate international lying, and the costs of unsuccessful lying may be excessive. But it is possible to lie and still avoid the reputa-

tion of being a liar; there are even some advantages in being known to lie. And incentives to lie increase in a nuclear age (p. 89).

Related, but not the same as lying, is ambiguity in signals, which makes it possible to "transmit information to some actors while concealing it from others" (p. 116). Ambiguity is achieved through noise in the communication system. It provides flexibility and protection for probes and initiatives (p. 123). There are, of course, costs and risks in ambiguity also.

Although signals are conventional and depend for their usefulness on their having a standard meaning, still it is possible by "coupling" and "decoupling" to create new signals or destroy old ones. They may be reinterpreted in different ways or old associations may be severed. The same fate may befall indices (pp. 142 ff.).

The most illuminating insight I derived from this subtle and sophisticated analysis is the utter absurdity we have allowed ourselves to fall into when we have to "waste money or otherwise harm" ourselves "to provide an index of [our] resolve" (p. 250). Whereas before the nuclear age we had to show our resolve by our ability to harm others, we now have to prove how much harm to ourselves we can take. One way to do this would be to kill a thousand people just to show how tough we were. Actually this is about what we do when we deflect limited resources to defense rather than to health, welfare, and education, urban redesign, and the like, for "money spent in support of foreign policy cannot be used to secure these other values and thus can be considered to represent a sacrifice in a way not dissimilar to killing one's own population. . . . The similarity is quite real" (pp. 251–52). People killed or wounded by neglect from their own government are just as dead and wounded as if by the military of some other government. Who is being defended?

Since a considerable portion of defense spending is primarily for signaling, not defense—a useless piece of hardware serves the purpose as well as any other (p. 93)—it should not be impossible to decouple this absurd form of image projection. Is it not possible that the deterrent potential of an image of a nation with no poverty, no avoidable illness, no ill-educated, no "hard core" of anything would be as great as the image of a nation with ten more ABMs?

Suicide and the Meaning of Civilization. By Thomas G. Masaryk. Translated by William B. Weist and Robert G. Batson, with an introduction by Anthony Giddens. Chicago: University of Chicago Press, 1970. Pp. xli+242. $10.00.

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It is not much known in the Western world that Czechslovakia's contributions to sociology can be traced to the founder of its republic, Dr.

Thomas G. Masaryk (and his collaborator, Dr. Eduard Beneš), especially since both of these scholars made their reputation as active politicians and statesmen. Yet, William B. Weist points out in his "Translator's Preface" that "an examination of the scholarly writings—as distinguished from the political, journalistic or polemical works—clearly reveal a man who anticipated in important ways some of the most influential contributions to the sociological knowledge of the present century" (p. xv).

In the "Introduction" (pp. xix–xli), which is also an excellent survey of Masaryk's academic and political career, Giddens tells us most convincingly that the originality of Masaryk's *Suicide* (published in German in 1881), was due to the author's "sociological interpretation of the phenomenon on the basis of the crucial importance of religion as a source of moral control in society" (p. xxiv). Giddens strengthens his high praise of Masaryk by placing his analysis within the framework of other well-known specialists in suicide (Durkheim, J. P. Falret, Guerry, Etoc-Demazy, Lisle, Winslow, Morselli, Caspter, Muller, and Wagner) and by showing how Masaryk agrees or differs with these authorities. It is important, especially because of subsequent trends in sociology, that Masaryk rather extensively used a variety of statistical researches related to suicide. He connects suicide with the conditions of social life, indicating that suicide varies by sex, marital status, occupation, and age; that rates of suicide are higher in urban areas than in rural ones; that they decline in times of war and rise in periods of rapid economic change, etc. However, Masaryk stressed that these factors are actually only symptomatic of the more deeply rooted malaise in the moral order which underlies the high rate of suicide characteristic of the modern age.

The reprinting of Masaryk's book (and the translation is very good!) is very welcome. Weist's "Translator's Preface" (pp. vii–xv), Giddens's "Introduction" (pp. xix–xli), and especially his section, "Masaryk's Academic and Political Career" (pp. xix–xxvii), are also valuable, since they are supported by the available materials and references to Masaryk's career.

It is quite possible that some specialists in the field of comparative sociology might doubt the somewhat excessive enthusiasm of Weist, Batson, and Giddens about the influence and impact of Masaryk's work on suicide. But we can also propound that their very enthusiasm has given us a small volume which can be evaluated as one of the best and the most recent studies of Masaryk's great career.

Natural Symbols: Explorations in Cosmology. By Mary Douglas. New York: Pantheon Books, 1970. Pp. xvii+177. $5.95.

Shlomo Deshen

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After Durkheim and Weber, the study of religion by social scientists was,

relatively speaking, unproductive for many years. Only since the mid-1950s has new ground been broken with a number of profound and original studies, many of them by British social anthropologists. Today, there is virtually a renascence, as the study of religion becomes one of the most exciting fields of research. So far, the new studies have mainly been monographs on particular religions. Mary Douglas, in *Purity and Danger* (London, 1966; Baltimore: Penguin Books, 1970) and in the book under review, has attempted to systematize these studies by approaching the data with new theoretical concepts.

In *Natural Symbols*, the author starts with the premise that the human body serves to express symbolically patterns of social relationships and social experiences. To compare the variety of symbolic action, she establishes criteria for classifying different social experiences. The fundamental variables used in classification are "grid" ("grid refers to rules which relate one person to others on an ego-centered basis" [p. viii]), and "group" ("the experience of a bounded social unit" [p. viii]). Focusing "on the concordance between symbolic and social experience" (p. 64), which Douglas calls "the replication hypothesis," she juxtaposes it to "the compensation theory" which treats the symbolic order as a secondary, purely expressive result of the social order, and asserts that "a replication hypothesis allows for the power of symbols generated in a particular social set-up to control it" (p. xiv).

While Douglas's basic ideas are familiar to students of religion, her contribution lies in her systematic development of them. By proceeding from highly abstract analytical variables—instead of the culture-bound variables so often used in the sociology of religion—the author's work becomes a stimulating cross-cultural study of symbolic action. In chapter 1 she dwells on religions that are more or less "ritualist." In contrast to analyses that proceed from the familiar dichotomy between the religions of "primitive" and "modern" societies, Douglas develops her analysis of types of religion within the universe of tribal societies. She arrives at the very interesting conclusion that some "primitive" religions are sometimes averse to ritual, and that the type of symbolic action they endorse can be analytically compared with such modern phenomena as Lutheran Protestantism (in contrast to medieval Catholicism). Douglas goes on "to relate [tribal studies] to ourselves" (p. 22). Drawing on the sociolinguistic work of Basil Bernstein, she relates the causes of anti ritualism in the contemporary western middle class to prevailing patterns of socialization and social control, and establishes parallels between them and comparable social structures in tribal societies. The author reinforces her argument with a case study in directed religious change in which the initiative of the religious innovators and the response of the laymen are analyzed within her framework. The argument "that there are pressures to create consonance between the perception of social and physiological levels of experience" (p. 70) that was presented in *Purity and Danger* is developed

here. Douglas now advances the hypothesis "that bodily control is an expression of social control—abandonment of bodily control in ritual responds to the requirements of a social experience which is being expressed," and argues that "co-varying attitudes in political thought and in theology" (p. 70) can be traced, such as "a religion of ecstasy as distinct from a religion of control." Relating her hypothesis to a number of current theories on religious movements, she verifies them through a comparative study of two African peoples.

In chapter 7 she examines the relationships between attitudes toward wrongdoing and the degree of formality and control in symbolic behavior. The author summarizes her discussion with the statement: "What is taken to be a more advanced, enlightened doctrine on the subject of sin appears merely as the usual expression of a less differentiated experience of social relations. We have here a glimpse of the sociological matrix in which ideas about sin and the self are generated" (p. 102) Having established a correspondence between social structures and ritual, sin and self, the author now formulates a typology of cosmologies which she develops in detail in three chapters. Toward the end of the book Douglas returns to the question of the nature of contemporary "anti-ritualism," and concludes "that the apparent anti-ritualism of today is the adoption of one set of religious symbols in place of another" (p. 166)

Addressed to a wide audience that includes religious practitioners, *Natural Symbols* is full of delightful, interesting, and provocative analyses on a variety of subjects: millenarian movements, student unrest, the personality of Lloyd George, the childhood of Sartre, Augustinus, and assorted tribal religions. The first chapter is particularly valuable and is worth the attention of teachers of comparative social institutions. (Douglas's brief article in *New Society,* March 12, 1970, is also noteworthy in this context.) Douglas also has a religiosocial message which should interest religious intellectuals. Yet her style and language detract at times from her precision. Had the author indulged less in colorful metaphor and more rigorously defined basic concepts, the impact of her book would have been greater. Crucial and recurring terms such as "symbol," "ritual," and "religion" are used as if their meaning were simple and straightforward. In fact, I am still unsure about the precise meaning of "natural symbols." Also, while the titles of the chapters are catchy and attractive (e.g., "Away from Ritual," "To Inner Meaning," and "The Bog Irish") they fail to help the reader to get an idea of the direction of the book. Nor for that matter is the title of the book a helpful guide.

Another unfortunate feature of the book is its insularity. Douglas does not refer to recent work that also attempts to systematize findings in the field of religion, namely that of R. N. Bellah, M. Gluckman, P. E. Slater, G. E. Swanson, and V. W. Turner. These studies, which touch in part on Douglas's concerns, are neither considered nor evaluated. Had she confronted these authors, her own analysis might have been more power-

ful. Even if she had only built more explicitly on the foundations of her earlier work, *Purity and Danger*, the present book would have benefited. American sociologists will be particularly astonished that Max Weber is not mentioned. Surely Weber's "problems of meaning" have something to do with Douglas's argument, and the concept might have been used profitably. Nevertheless, *Natural Symbols* is an important book, worthy of the monographs on which it is based.

Some minor points: there are typographical errors on pages 32 and 122. The last paragraph on page 81 is unclear, and the bibliography does not seem to have been checked carefully.

Studies in Taiwanese Folktales. By Wolfram Eberhard. Asian Folklore and Social Life Monographs, vol. 1, Lou Tsu-k'uang, general ed. Taipei: Orient Cultural Service, 1970. Pp. 193. $3.50 (cloth).

David K. Jordan

University of California, San Diego

This book reports on a study of folktales by the author and his assistants in Taipei city. Eberhard is specifically concerned with several questions that lie somewhat outside the usual range of folklorists' interests, but are fundamental questions that a social scientist would ask in studies of folktales: If we say a story is Chinese, how do we know who knows the story? Some Chinese? All Chinese? A few Chinese littérateurs? If a story is transmitted from one generation to the next, is there a tendency for the children's version to resemble the version of their parents more than it resembles other versions in the community? Does the history or origin of a tale affect the answers to these questions?

Eberhard collected 349 texts from as many respondents, asking them to tell any one of four proposed stories or another story. The majority of the stories, and the bulk of his analysis, are based on two tales, one concerned with a tiger who devours one sister and is undone by the other in a kind of Chinese Little Red Riding Hood story, and the other concerned with a boy who appears from a peach pit and slays a monster who is terrorizing the countryside. Eberhard divides his raconteurs into fathers, mothers, sons, and daughters, and divides his households into four educational classes. He also administered Thematic Apperception Tests and California Personality Inventories to the tale-tellers, but this book reports only briefly on these studies, since the data are still under analysis. His concern with the importance of the teller's personality influencing his telling of the tale is laudable and should produce interesting results in time.

Eberhard discovers that every story has a "core" of diagnostic elements which define it. Those elements which he finds in a particular variant that are not part of this core are construed as belonging to a softer, more

flexible "shell" of embellishments, "making the story more dramatic, more appealing, or more frightening, but which are not essential to the plot and can be left out or modified" (p. 164).

Since Eberhard worked with unskilled tale-spinners, some of the variants collected were extremely fragmentary. Accordingly, the "core" version was construed as consisting of those elements which occurred in more than an arbitrarily assigned percentage of cases. This is not an ideal procedure but is acceptable given the questions that the research is designed to answer. The study finds enormous variation in the shell elements of a tale even when elicited from respondents in the same community, or (somewhat surprisingly) even in the same family. The categories of age, sex, and social class (defined by education) do seem to be significantly related to the selection of particular shell elements. The parent-child relationship does not. And sex and age seem more important than class background.

The book is enriched by several discussions not directly related to its main objectives. A discussion of which tales respondents enjoyed telling and which they did not is revealing. So is a consideration of what stories people think ought to be told to children and why. An attempt to analyze two variants of the same tale by Propp's method demonstrates that they would have two different structures in Propp's scheme, and defends the author's opinion that a knowledge of variants is crucial to an understanding of the tale. Chinese versions of several variants of each of the two principal tales are included by way of an appendix. (Since the book is printed in Taiwan, one could wish that Chinese characters also occurred at various points in the text, as in bibliographic citations, but for some reason they do not.)

There are several points where a reader may have misgivings (ranging from the printer misspelling the author's name on the cover to Eberhard's casual remark that "historical reconstruction must proceed . . . along the lines of logical speculation" [p. 166]), but the general thrust of the book in trying to discover the social factors at work in the perpetuation of the tale in a given community involves questions which the social scientist must immediately agree are important if our knowledge of how society works is to benefit from studies of the folktale, or if our knowledge of how folktales work is to be improved by studies of society.

The Red and the White: Report from a French Village. By Edgar Morin. Translated by A. M. Sheridan-Smith. New York: Random House, 1970. Pp. xxiv+263. $7.95.

William Christian

University of Michigan

In 1966, when the writing of *Chanzeaux: A Village in Anjou* was com-

pleted, those of us involved in the project thought that we had completed a thorough study of a very traditional village, and had caught the essence of the changes taking place there. A year later many aspects of the commune had changed beyond our wildest imagining. Television was everywhere; teenagers with long hair were to be seen in the town square on Sundays; the villagers were struck with a compulsive wanderlust that took them on trips to Spain and Austria; and some of them had even been to a rock festival on the Isle of Wight.

Edgar Morin's book, written at the same time about a village in Brittany, manages to capture this sudden bursting of the rural chrysallis and to accord it careful, thoughtful attention. Morin's care for the particular details of change is coupled with a philosophical predisposition to generalize; the result is felicitous. Morin largely succeeds in his impossible aim to write both about the village and about France. Indeed, he is writing both about the village and modern Western society in general.

The English title, which refers to two political traditions in the commune, is misleading. The main thrust of the book deals with modernization, a process that is rendering the red and the white parties increasingly irrelevant. The white (or Catholic) tradition is treated in only one chapter because since 1876 the red tradition (that of secular republicanism, and later communism) has been overwhelmingly dominant.

After giving an overview of Plodemet in the midst of modernization, Morin and his fieldworkers report on its major victims: the farmers; on those who have enjoyed change the most: the very old and the young; and on the secret agents of change: the women. The last two chapters, dealing with the villagers' changing philosophies and cosmologies, are particularly penetrating. They ring true for villagers I know in Spain, and indeed would logically apply to peasant villages throughout Western Europe. Morin is particularly skilled at perceiving the world views behind what most anthropologists would shrug off as the cracker-barrel clichés of the moment. In his analysis of the changing attitudes toward nature, space, and time, and in his discussion of the progressive liberation first of the family and then of the individual, Morin transcends the scope of his village study and brings us to a clear, even sympathetic understanding of the bourgeois mentality. The society of consumption is placed in a historical perspective. We learn about ourselves by watching, as privileged guests of a perceptive observer, the emergence of those bourgeois values that, though new to the citizens of Plodemet, are old (and increasingly incomprehensible) to many of us in America. Morin is describing, in its incipient form, the American way of life.

Before I could enjoy the book I had to get over my distaste for the author's style, which runs to long words and precious, paradoxical phrasings. We are also subjected to a somewhat pompous reification of his field approach ("The Multidimensional Method") which is surely superfluous. But these are minor matters.

The Red and the White is not a study of a typical, traditional Breton

village. Plodemet is not particularly traditional, as Breton villages go, because of its red past and its concentration on secondary schooling. For the same reasons it is certainly not typical. Nor does the study of Plodemet shed much light on Brittany or the Breton movement, because the Breton movement was predominantly a white movement, and Plodemet was resolutely red, and thereby centralist. Nor does this book give a complete, detailed view of a village social structure. Virtually no numerical data are given at all, and the seamen, the church, and the patterns of migration, temporary work, and exchange with the outside world, are given comparatively short shrift.

The book *is* an illuminating discourse on the direction of modern Western styles of life, anchored in the meticulous assessment of trends in a French village. It is a very successful study. In its own uninhibited way it is unmatched.

Irish Life in the Seventeenth Century. By Edward MacLysaght. New York: Barnes & Noble, 1969. Pp. 480. $17.50.

Karl S. Bottigheimer

State University of New York at Stony Brook

First published in 1939, revised and enlarged in 1950, and now reprinted with minor revisions, this is still the best, if indeed virtually the only work on the subject. Dr. MacLysaght, the dean of Irish antiquarians, has probably cast his eye over more written records of seventeenth-century Ireland than any man living, and he writes about his country's history with assurance, vigor, and a pride mingled with compassion. The continuing value of his book, however, derives not from what it says but from what it contains. The reader will search in vain for a central thesis, but will be pelted by miscellaneous social information, much of it from manuscript and Gaelic sources still relatively inaccessible. Some of this is patently trivial. What the average peasant had for lunch is not, in itself, very significant. MacLysaght's purpose was not to separate the significant from the trivial, but to describe society as it could be reconstructed from the surviving accounts of seventeenth-century men and women. What *he* writes about is what *they* wrote about, and the result is a marked predominance of description over analysis.

In some ways the title is a misnomer. Not for 800 years (and possibly never) has there been such a unitary thing as "Irish Life" in Ireland. Nor is there any more prominent characteristic of seventeenth-century Ireland than the duality created by an English, Protestant life in the upper classes, the professions, the government and the established Church; and an anticulture of Irish Catholics on the land, in the wilderness, in exile, or in menial occupations. "Indeed, Madam," a correspondent wrote to the Protestant Countess of Orrery, "I think it is dangerous in these times

[1672] to keep a Papist butler, though he is a good, serviceable man" (p. 290). At the very least there were *two* "Lives," and despite his outspoken personal identification with *one*, MacLysaght makes an honest effort to treat both. Some memorable vignettes are the result: the long suffering traveler whose efforts merely to get himself across the Irish Sea took seven weeks of the summer (p. 265); the Catholic priests trying vainly to control "keening," the unrestrained lamentation over the dead (p. 286); the Quaker who, in 1669, "stripped to the waist . . . entered a Catholic Church during the celebration of Mass, carrying a dish of burning coals and brimstone on his head . . . [and] denounced the superstitious idolatry of the congregation upon whom he had thus dramatically intruded" (p. 297). The overall picture is that of a colonized, divided, deeply riven island; and there are many to argue that it remains so today.

There are a few minor faults. In a book costing $17.50 the quality of production is shocking. Two pages fell out of the review copy, and several others were so blurred as to be scarcely legible. It is also possible to lose patience with Dr. MacLysaght's occasional pedantry. His refusal to translate Irish quotations into English is vexing, and at one point (p. 168) he devotes several paragraphs to the subject of *creaghts* (a sort of Irish shepherd) without bothering to say what they were (though a footnote refers one to a scholarly journal for further information). But these are perhaps forgivable pretentions in a scholar who was struggling to assert the Irishness of Ireland's past against a lingering presumption of the insignificance of "Irish Life," which was (and to some degree remains) a distorting and degrading part of the colonial legacy.

Italian Americans. By Joseph Lopreato. Ethnic Groups in Comparative Perspective, Peter I. Rose, general ed. New York: Random House, 1970. Pp. xiv+204. $2.95 (paper).

Sydel F. Silverman

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This book is the first volume in the series, "Ethnic Groups in Comparative Perspective." According to the series editor, Peter I. Rose, it is designed to give student readers two kinds of messages: those which can help him to know—that is, information about the origins and experiences, the cultural patterns, and intergroup relations of various ethnic groups—and also those which can help him to feel, viscerally, the ethnic experience. The editor sees the series as a necessary corrective to those approaches that write the history of ethnic groups as a series of great events enacted by great men, which always seem to run curiously parallel to the history of the dominant group. This, then, is to be another kind of history, that of everyday life. In this aim the editor is surely correct; the history he speaks of is the history that is sorely needed, and not only for the study of ethnic groups.

Yet one must ask how such a history is to be written: what kinds of questions are to be asked? To judge by this volume, there is no dearth of material in which to find answers; it is the guiding purposes of inquiry that are elusive. Lopreato's task is a global one, in effect to review (in not many pages) the sojourn of the Italians in America, and he makes a noble attempt at it. There are many facts to cover, and he covers them well enough; there are many points to make, and he makes them convincingly. Yet one misses the sense of what he is looking for in all of it. What, after all, does he find theoretically interesting about Italians in America? The difficulty with an effort like this is that in saying so much, it can end by saying very little. The "facts" are all there, but without applying them to the impelling theoretical problems they leave one without a real grip on the Italian experience. Nor can it be expected that the series as a whole, once completed, will yield theoretical purpose simply by setting side by side parallel chapters on several ethnic groups.

Still, considered as a concise text, the book does its job. It begins with a general discussion of the problem of immigration and the gross history of immigration in America. It then devotes a dozen or so pages, a chapter called "In the Home Society," to the historical, geographical, and social background of the immigrants. That the subject is vastly oversimplified goes without saying, but the problem seems compounded by an unfortunate decision to phrase much of the discussion in terms of "the typical southern village." A chapter on "Patterns of Settlement" follows, tracing the formation of ethnic communities in the United States.

The chapter "Social Institutions and Change" is primarily concerned with the family, reviewing the studies of Caroline Ware, Paul Campisi, Irvin Child, Herbert Gans, and others on types of Italian-American families. It also contains short discussions of Italian-American Catholicism and the *padrone* complex that characterized early immigrant labor patterns. The choice of "social institutions" for inclusion in this chapter is a curious one. Most glaring is the omission of any discussion of friendship, patronage (in the broad sense), neighborhood, intragroup politics, or other network phenomena that are so crucial to an understanding of Mediterranean-based societies.

Two chapters document "Intergroup Relations" between Italians and other groups in American society and "Assimilation and Achievement," the overall scope of Italian-American socioeconomic accomplishments. A concluding chapter ponders the general process of assimilation. Considering the probable future of Italians in America, Lopreato weighs the evidence for a stable cultural pluralism as against continuing assimilation and seems to conclude that the latter is most likely.

Throughout, Lopreato includes human-interest pieces—excerpts from interviews, literary works, and the like. These add some color, but for the most part they are too much out of context to make the reader genuinely "feel" the Italian experience. Despite its less than total success, this effort to integrate the sociological and humanistic dimensions is a hopeful sign

in the textbook field. Further hopefulness is engendered by the near absence of jargon.

Lopreato is at his best when he is exploding social science myths about Italian Americans, and the poppycock notions do indeed abound: that the immigrants were peasants with a mystical attachment to the soil, that phenomena observed in working-class ghettos adequately represent second-generation Italian-American life, that Italians have a low "need for achievement" and that they eschew formal education, to name only a few.

Certainly, if one wanted to teach a course on American ethnic groups and wanted to include Italians in it, this book would be a reasonable text; it provides an authoritative, well-written summary of their epic. But this reader was left with the suspicion that sundry summaries of epics may not inform very much, unless we have good questions to ask of them.

Prejudice U.S.A. Edited by Charles Y. Glock and Ellen Siegelman. New York: Praeger Publishers, 1969. Pp. xxii+194. $5.95 (cloth); $2.25 (paper).

Peter I. Rose
Smith College

At first glance this volume seems anachronistic. Here a group of sober-minded and identifiably liberal spokesmen offer analyses of discrimination and plans for enhancing integration. At close examination one learns that, indeed, many sections of this volume *could* have been (and in many ways *were*) written in the 1950s.

Here we have an impassioned introduction by Father Theodore Hesburgh who talks of categories and stereotypes and prejudices learned in families, neighborhoods, schools, "even, alas, our churches," followed by Saunders Redding's recounting of the struggle for equality in which varied routes are mapped out and considered. Like Father Hesburgh (and, it turns out, everyone else whose works are printed here), Redding argues for an "inclusive American community" (his phrase) which gives freedom to all—and allows each to do his ethnic thing in the spirit of "pluralistic unity" (my phrase).

The rest of the book consists of several rather uneven reports on prejudice in politics, in the churches, schools, media, and marketplace, many of them presented at a symposium held in Berkeley in 1968. These, too, are period pieces. As contributions to sociological understanding I would rank them in the order cited (which is not the order presented). In fairness, it should be noted that the last two named were written by Dore Schary and Howard J. Samuels, neither a social scientist. Their selections are straightforward reports on what they know best—the media and the marketplace.

The two articles on the schools are, each in their own way, interesting

but limited. M. Brewster Smith offered some conclusions from his recent study on three communities. Among other things, Smith found that even in the absence of formal discrimination (as in the integrated school) prejudices persist, especially anti-Negro prejudice. Charles Silberman, in the other selection, tells about what schools might (but fail to) do about such facts of social life, especially when, "to put it bluntly, we simply do not know how to educate youngsters from the lower-social-economic class." He goes on to explain that the failure Smith pointed out is "moral failure" and teachers, Silberman feels, cannot avoid moral education. "*What* we teach will be less important for the purpose at hand than how we teach; what we do speaks louder than what we say."

Not all is cliché or truism, however. Silberman has much to say about what we teach and what to do. This is clearly evident here and, especially, in his recent book, *Crisis in the Classroom* (a volume destined to prove as big a bombshell as his first "crisis report").

Stark and Glock raise some interesting questions about religion and politics. And it is obvious that they are giving but a smattering of what they have learned from a series of studies on the issue. (Every other footnote announces another volume in progress on some aspect of the subject. One eagerly awaits their publication.)

Lipset's essay is by far the most interesting. In it he discusses and analyzes the appeal of right-wing political movements to the "once had" generations of Americans dating back to the founding of the country. In rapid succession he begins with a discussion of Smelser's ideas about social movements and then reviews the history of what was so recently called "the radical right," from the Anti-Illuminati Movement, through the Know Nothings and the APA, the Klan, the Coughlinites, the McCarthy Period, to George Wallace. Reading that paper (which comes early in the volume) I had the desire to turn from *Prejudice U.S.A.* to *The Politics of Unreason* (Lipset and Earl Raab's recent book from which this essay was excerpted). Still, as a reviewer, I went to the next topic and the next. Each of the better pieces left me with the same sense of incompleteness.

Laymen (to whom this book seems primarily directed) may find that what is here is just enough. Readers of this *Journal* will want more. I would hope that *whoever* reads it will be prepared to ask how the editors of *Prejudice U.S.A.* (or any of us) can continue to include that predictable last chapter on "Toward Control of Prejudice and Discrimination in American Life," complete with the same panacea ("commitments," "blueprints," the "harnessing of resources") that haven't cured anything.

The problem is not a lack of goodwill or even programs, but the fact that good liberals often prove to be rather poor policy advisors. Too often they (we) seem to fail to take into account that which sociologists should know most about. How does our knowledge of prejudice and power and politics and pressure get translated into action? How can we break into the system that is constantly alluded to but rarely discussed?

Nathan Glazer published a perceptive and troubling book recently

called *Remembering the Answers*. I would like to see another, on the topic under discussion, called "Remembering the Questions."

Japanese Society. By Chie Nakane. Berkeley: University of California Press, 1970. Pp. xi+157. $5.00.

Harumi Befu

Stanford University

Nakane's basic argument is simple: Japan has a hierarchical social structure which has not changed for centuries. The structure takes the form of an inverted V, with one leader and two followers. Since the link between the two followers is negligible in her formulation, I suggest simplifying the structure to an I, with one leader and one follower, as Nakane herself ends up doing near the end of the book (p. 141). The relationship between these two individuals is characterized, according to her, by affective personal ties and authority of the leader and loyalty of the follower. Increase in the number of followers does not affect this relationship, except that in a group of several members there tends to be relatively little internal functional differentiation. And an effective group tends to be a jack-of-all-trades, as seen in the giant *zaibatsu*. For her, the Japanese box lunch (called *makunouchi*) which contains an assortment of foods and universities with many colleges are veritable illustrations, not figures of speech, of the same tendency—an assertion of dubious worth.

Nakane admits some of her exposition is not exactly new (p. viii), and indeed what she has to say has the pungent smell of familiarity. Have we not heard some of this from Ruth Benedict, from Bellah? Was not the basic theme stated in John W. Bennet and Iwao Ishino (*Paternalism in the Japanese Economy* [Minneapolis: University of Minnesota Press, 1963]) and in my own work ("Ritual Kinship in Japan," *Sociologus*, vol. 14, no. 2 [1964])?

There is merit in working out one overarching structure of the society, as Nakane does, applicable to a variety of groups and institutions rather than seeing the society composed of discrete groups and institutions, each of which requires a separate descriptive account. However, in her zeal to account for Japanese social structure over many centuries with one basic structural principle of verticality, she has to simplify Japanese history to an extent that is a caricature. The role of the traditional Japanese family is said to be played now by business firms (p. 8). The landlord-tenant relationship of rural Japan of the past is equated to the modern industrial management relationship. The samurai mentality of the feudal era, which she characterizes as one of sublimation of the sexual interest in the wife to devotion to the lord, presumably still persists in the modern salaryman

"to some extent" (p. 71). That "to some extent" there is persistence of social structure from premodern times no one would deny. The important question is exactly to what extent and precisely in what respects there has been structural persistence. To answer this question by offering an inverted V and invoking a vertical principle (p. x) would enlighten few Japanologists.

What bothers me most about this book, however, is not so much Nakane's interpretation of Japanese society as her lack of a conscious theoretical and methodological approach. She disarms the reader by proclaiming that she uses no known sociological method or theory, that hers is the intuitive approach of an artist, and that the book is not a scientific treatise (pp. vii–viii). In my mind, such a disclaimer does not give her immunity from rigorous scientific criticism. It does not excuse her for making such a statement as: the social stratification of Tokugawa Japan, with samurai as the ruling class, had "no sociological significance in the matter of the total social configuration" (p. 42).

She says she has used anything available which seemed to be effective in bringing out the core of the subject matter, and thinks that this approach might be closer to that of the social anthropologist than to that of the conventional sociologist (p. viii). If haphazardness is the method of social anthropology for her, it is only fair to inform the reader that it is not the method for many others.

A closer analysis reveals that Nakane is not without method or theory. As she stated in an earlier Japanese essay (1964) on which this work is based, she takes the approach of British social anthropology. This is quite evident in (1) her basic structural-functional approach; (2) her concept of social structure as interpersonal relationships which is straight from Radcliffe-Brown; (3) her static conception of social structure, *à la* Firth; (4) her treatment of social structure *qua* social structure, not showing due consideration for the causal roles of economic and psychological factors; and (5) her concept of the "vertical principle," methodologically reminiscent of Radcliffe-Brown's "principles" of lineage solidarity and of sibling unity. Trapped in a functional approach which does not readily admit analysis of change, she was forced to see Japanese society in static terms and argue for a rigidly unchanging structure even in the face of the bewildering transformation of the society in the past few centuries. Firth was able to account for change with his concept of social organization; Nakane failed on this score.

In spite of all I have said, there is one unwitting contribution which Nakane's book makes. In her analysis of the vertical structure in Japan, she makes clear that the leader generally combines both instrumental and expressive roles in one person, *contra* Parsons, who has maintained that a small group must have two separate persons to perform these two functions, a point which is well worth noting. I say "unwitting" because Nakane is apparently unfamiliar with developments in American sociology, as pointed out by Robert Marsh in his review (*Minzokugaku*

Kenkyu, 1970) of the Japanese edition (Nakane 1967) of the book under review.

If I recall correctly, several years ago Nakane complained that Japanese scholars are unable to take criticisms from their colleagues, making book reviews rather dull reading. I trust Nakane can transcend her cultural bind and read this review with dispassion.

Social Change and the Individual: Japan Before and After Defeat in World War II. By Kazuko Tsurumi. Princeton, N.J.: Princeton University Press, 1970. Pp. xiv+441. $15.00.

Hiroshi Wagatsuma

University of Pittsburgh

This book is a collection of very interesting descriptive studies of various aspects of pre- and postwar Japanese society (with some good insights), which is, unfortunately, spoiled by a spurious overlay of sociological concepts and models.

During the 1930s, under the severe thought suppression by Japan's militarist government, more than 95 percent of about 500 leftist authors are said to have undergone ideological conversion. In chapter 1, Miss Kazuko Tsurumi presents case studies of such conversion (and nonconversion) experiences of seven well-known intellectuals. Although very stimulating, the analysis does not go deep enough into the personality dynamics of each individual, and this may, at least partly, have been caused by the somewhat mechanical typology into which the author tried to "fit" the Japanese authors. An important question remains unanswered: isn't there anything uniquely Japanese about the inner experiences of the majority of these intellectuals who, under threat and torture, failed to maintain their opinions and beliefs?

Chapter 2 is a brief descriptive history of the conscription system and the nature of the military education in prewar Japan. Chapter 3 is an excellent analysis of military indoctrination of patriotism based on the Emperor cult. The author skillfully quotes from letters and diaries of college and university students drafted into military life during the war to illustrate how the army achieved its educational goal by isolating the soldiers from their ordinary life, depriving them of privacy and by using violence. Chapter 4 is the study of the experiences of Japanese war criminals, with ample quotations from the published collection of personal documents (letters, diaries, essays, and poems) left behind by 701 individuals. It offers fascinating reading, but the author remains basically descriptive, and, for instance, her treatment of guilt feelings among the Japanese is inadequate. Also I wonder why she did not comment on some of the possible reasons so many Japanese compose poems on their deathbeds.

Chapter 5 is a brief but interesting description of changes in socioeconomic structure of postwar Japan. Materials presented here point to more than "the transformation of a predominantly communal-totalitarian society into a predominantly communal-mass society" (I would doubt the value of such a dichotomous view of social change). Chapter 6 describes an interesting social phenomenon: a circle, or small voluntary informal group of textile workers, formed to write autobiographical essays. The chapter does not really show the "process of personality change" which the author promises to trace, but it certainly invites the readers to a further study of this subject, because there is something very Japanese about writing autobiographical essays and sharing them within a small group of intimate friends. Chapters 7 and 8 are mostly based on the similar kind of personal essays written by Japanese women, concerning their feelings about the war, about their children, their own mothers, etc. Both chapters are extremely interesting to read and provide us with additional information of considerable value on Japanese women's inner experiences.

Chapters 9 and 10 present the opinions of the "activist," "interested," and "apathetic" students, and the comparison of the contemporary students with those during the 1930s (based on questionnaire survey and interviewing). Unfortunately, nonactivist students are underrepresented in the sample and we do not know how much we can generalize on the reported interesting findings. Chapter 11 is a collection of very brief (too brief) portraits of six student leaders, which could have been much more informative if the author had collected more detailed life history materials and some psychological test data from these students.

In short, the book is of primary value as a rich source of provocative materials that invites its readers to further studies of various important subjects. As such it is to be highly recommended. Its theoretical value is questionable. The models presented in the introduction and elsewhere are not very useful. The author is serious about her theory building, or model using, and yet discrepancies are often created between what she proposes to accomplish theoretically and what she actually does empirically. She also seems to be trying to tackle both sociological and psychological factors in her study of social change and yet she does not really demonstrate the interplay of these two sets of factors in a structured way. Treating prewar and postwar Japan as almost totally different societies, the author seems to be using a theoretical model of social change based upon a dichotomous view of societies and a unilinear process of social development. Or, while referring to adult socialization and personality change, she is often dealing with no more than attitude and attitude change. It is unfortunate that the book had to be published with all these (and other similar) weaknesses. It would have been a much more enjoyable book both for its author and readers without its conceptual "frosting," which the author does not need at all in order to make her every single important point.

Technological Growth and Social Change: Achieving Modernization. By Stanley A. Hetzler. New York: Praeger Publishers, 1969. Pp. xi+302. $7.50.

Manning Nash

University of Chicago

The study of modernization, economic development, or industrialization has always had the peculiar ability to attract global sorts of explanation and analysis. Frequently the attempts to account for modernization have been reduced to a monofactorial mania. Hetzler's book is the latest in this line. The ancestry is shown by the rather capable way he treats his predecessors in this endeavor.

He reviews the inability of hypotheses about capital formation and entrepreneurship to shed much light on the process of modernization, and similarly takes to task some partial hypotheses in sociology, social psychology, political science, and in anthropology. Having shown that the social sciences as disciplines and as a whole have not developed a satisfactory theory of modernization, he sets out to chart his own brave new world of concept and hypothesis, capped by a series of developmental recommendations based upon his analytics.

The account begins with half an insight, undisputed in contemporary social science: the basis of modernization, in the last analysis, is the continual supply of fresh technological inputs as they are applied to every branch of production. This assertion is common—Marion Levy, Jr., Simon Kuznets, Julian Steward, Leslie White, myself, and a host of others have often in print assented to it—but apparently Hetzler thinks it novel. The reason this is but half an insight is that the social, cultural, and psychological conditions which promote or facilitate both the development and employment of new productive technology are still problematic. In a sense, that is what the social sciences are engaged in studying in the process of modernization.

Hetzler develops a series of stages of technological development and a vocabulary for analyzing "socio-techniques" or man-machine relationships. The stages are but labels for the history of Western technological growth. They have neither logical, causal, or thematic connections. When some mechanism is needed to explain why this sequence happened once, and is likely to happen again, Hetzler falls into one of the social sciences' larger solipsisms: "Technology is a socio-technological entelechy containing the seeds of its own growth." Shades of the 1920s and W. F. Ogburn! To compound this he envisions cultural and social ability to adapt any technology, and finally he sees a world in which the man-machine relations eventually dictate not only uniform technological methods, but uniform social practices.

With this jerry-built scheme around half-understood social science, the planning recommendations are not as bad as might be expected, but still they are recipes truly divorced from the real fractious, unstable, and striving kitchens in which they must be cooked.

Future Shock. By Alvin Toffler. New York: Random House, 1970. Pp. xii+505. $8.95.

John Greenway

University of Colorado

Go and catch a falling star, get with child a mandrake root, and if these futilities are not sufficiently challenging, write a book about what still lies beyond time's terrifying arrow. In projecting the future there is only one certainty: one plays the fool for posterity, and in these accelerative days the fuse of a prophet's petard is suicidally short. What was the future in Alvin Toffler's manuscript is already the unforeseen past in his published book. His outcast pacifist Cassius Clay metamorphosed to Abdullah Bulbul Amir and has been persuaded by the society he would not defend to take up arms for no more than forty-five minutes for a guarantee of $2.5 million. The prisoner of the Vatican put down his seven-tiered crown, put on a double-breasted white suit, and joined the jet set. Princeton University conferred an honorary degree upon drug-singer Bob Dylan; Phi Beta Kappa is considering opening membership to any "lover of learning." History is being rewritten as fast as history can be reread: Matthew Henson holds Massa Peary's mukluks no longer, but stands in his own at the top of the world, the newly discovered discoverer of the North Pole. Reveille will cease to disturb soldiers coming down in the morning, and the new chief of naval operations signals the future by presiding over the unsurrendered ships of the navy in hippie-length hair, which Toffler thought moribund. Twiggy has—Twiggy? Twiggy Who?

But *Future Shock* is only rhetorically about the future, so these anachronisms do not affect its importance to the social scientist as well as the layman, who has made it a best seller. Its subject is really what we know in an undifferentiated sense as culture shock, but extended brilliantly from its traditional synchronic dimension to a diachronic concept. Anthropologists have studied group psychological trauma almost exclusively as a phenomenon occurring when an advanced civilization collides with a backward society; but with its primitive competitors destroyed, the advanced society is in shock from colliding with its own advancement. Toffler's thesis is not thoroughly sound, however; by drawing his data from his own society, he frequently mistakes vagaries for verities. He is correct in seeing our future as a busted play with Americans scrambling frantically for a way to be saved—but culture shock is lethal only to a society whose soul is corrupt. My own experience studying the Paleolithic aborigines of Australia brings into question the principal thesis of *Future Shock*—that the limits of change the human organism can absorb are discoverable and are therefore controllable. I have seen these Old Stone Age people move ten thousand years in a day with scarcely any culture shock, for the soul of their society is strong. Anthropology, with a rich storehouse of evidence from hundreds of cultures, could have given Toffler the comparative information to improve his ideas, but the study of man has been

allowed to slip into the hands of mere bearded youths who waste the substance of their heritage by sifting coprolitic nuggets from the middens of their minds to build therewith a new anthropology. They no longer admit qualitative differences between cultures, so they cannot show why one society succumbs to the shock of change while another survives. The anguish of our own state they attribute to sins we have committed upon the weak, the poor, the "ecology," and the nonliterate peoples with whom we share our mischievously polluted planet.

In the most important last section of his book Toffler shows that this is nonsense, that the most serious credibility gap is academic rather than political. Our intellectual guides tell us we must reconstruct the world they say we have ruined. Toffler argues that the only remission for uncommitted sins is in changing ourselves. Without guidance our efforts to make accommodations with this impossible situation lead us into the crazy behavior the first half of *Future Shock* entertainingly describes Toffler does not pretend that by taking thought, lobsters can grow wings within a year and fly, but he does show how each of us can accomplish a commensurate miracle—to adapt ourselves to cope with a world we cannot change. If this sounds like Eric Hoffer, it is because *Future Shock* is the most perceptive analysis of the ordeal of change since *The True Believer,* though these two social analysts disagree about the path to our salvation. Toffler believes social evolution to be irreversible, like biological evolution, but I have been into the past, and have seen the aborigines of Australia's western deserts remain fundamentally in the Old Stone Age after two centuries of violent contact with the future. They take the new and imprison it in the past. So too may we, when we have had our fill of prophets urging us forward. Perhaps next year in Jerusalem, when the Pope appears in a Nehru jacket?

Work Organizations: Major Theoretical Perspectives. By Curt Tausky Itasca, Ill.: F. E. Peacock Publishers, 1970. Pp. x+223. $3.95 (paper).

W. Richard Scott

Stanford University

This slim volume is designed to introduce beginning students to the "major theories of organization, processes, and pertinent research, without glossing over areas of controversy" (p. vii). In the introductory chapters and in a concluding statement, three theoretical traditions are briefly reviewed: the classical approaches of analysts such as Taylor, Fayol, and Weber; the human relations perspectives of Mayo, Argyris, and Likert; and a heterogeneous grouping labeled the "structuralists" which includes the work of Woodward (technology and structure); Boulding and Katz and Kahn (open systems); and Simon and Cyert and March (decision making). Although important differences distinguish the classical from the human relations perspective, the most important being assumptions

about human nature and the bases of motivation, they are viewed as similar in their search for factors by which to increase the productivity of the work force and in their assumption that high satisfaction of participants and high organizational productivity are compatible goals. The structuralist perspective, to which the author is sympathetic, recognizes that these two goals may at times be irreconcilable.

Three additional chapters are largely devoted to spelling out the inconsistencies which may develop between individual and organizational goals. A chapter on tension management reviews various sources of stress and conflict: frustrated self-actualization; uncertain and unequal rewards; organizational change, particularly, automation; and factors affecting satisfaction with job content and job environment. A chapter on organized inequality takes up the problems of power and of setting limits to obedience to authority, the differing meanings of mobility to participants, hierarchical blockages to communication and intelligence gathering, and mobility among firms as a function of the prestige rankings of organizations. The final chapter of this set reviews the changing foci of research on the determinants of productivity, ranging from leadership factors to group norms to participant satisfaction.

In his preface, the author notes that he is skeptical about the human relations perspective; in particular, that he does not share their optimism about human nature. Rather, he embraces the assumption that man is inherently self-centered and indifferent to organizational needs. This assumption together with the notion of inconsistencies between participant and organizational goals provides the leitmotiv of this volume. The theme is not systematically developed so much as it is stated and then illustrated by a series of unrelated examples drawn from the literature. Beginning students may have some difficulty in following an argument that moves, for example, from Weber's typology of authority to consider three theories of mobility, then to a discussion of organizational intelligence operations, and ends by considering the concept of organizational sets. More advanced students, especially those who have sat at the feet of Paul Goodman or are anticipating the rapid "greening of America," will find the arguments of this volume provocative, if not compelling.

The Sociology of the Blue-Collar Worker. Edited by N. F. Duffy. Leiden: E. J. Brill, 1969. Pp. 193. Gld. 32 (cloth).

Arthur B. Shostak

Drexel University

This may go down in the annals of pop-sociology as the year of the Hard-Hats (1968 was the year of the Poor; 1969, the year of Black Power; 1970 the year of Student Unrest). Timely, then, is a 1969 hardcover edition of the *International Journal of Comparative Sociology* preoccupied with the sociology of the blue-collar worker.

Distinguished in the caliber of its international contributors, and in the rarity of the undertaking, this essay collection commends itself to al with a taste for cross-cultural reflection, blue-collar prospects, or the testing of general social theory. Especially rewarding is the considered attention to job-satisfaction components, the socialization of new migrants to industrialization, the role of trade unions, and the peculiar adaptations forced on industrialization by native conditions. Outstanding in this connection are essays by David Chaplin on Peruvians who are provocatively "not recapitulating Western development," and by Paola Ammassori on Italian blue-collarites: "This is the first time in the sociological literature that such an analysis of joint influence of family occupational tradition and personal work experience on the blue-collar workers' attitudes toward factory work and occupation has been presented" (p. 15).

Much can be learned from the anthology about the state of current research into blue-collar lives—though far less about blue-collarites themselves: moreover, much of what can be learned is not always what the editors or contributors may have had in mind. Consider, for example, these items: None of the eleven essayists thought it necessary, at even one point in 193 pages of prose, to quote from a living, breathing blue-collarite himself. Are we so far removed from the Oscar Lewis/Studs Terkel lesson not to understand the invaluable piece of reality represented by the language of our respondents themselves? Similarly, few of the eleven essayists seem widely familiar with anything but American sociological literature—and that of their own particular country. Are we so far removed from the lesson of other more cosmopolitan social sciences not to appreciate the light that British theorizing might shed on Argentinian issues, or that Indian reflection might shed on Peruvian matters?

That the blue-collarite exists in some kind of relationship vacuum, was the dizzying impression one got from essay after essay (save the Italian and East African pieces). Nowhere in the anthology is the worker's wife so much as mentioned, nor his children or peer-group relatives. Yet there is reason to believe his peer-group relatives are *the* critical reference group, his progeny are *the* rationalization for a "life of quiet desperation," and his spouse is *the* "anchor in reality" for most (Freud's nomination here of "work" notwithstanding).

Other critical dimensions of the blue-collar reality, far less decorous than the above, are also conspicuous by their absence. Blue-collar fanaticism about commercial athletics, cynicism about the "greening" of anything, frustration with emotional expression, reliance on pornography and sado-masochistic fantasies, . . . all these and more, are nowhere to be found in this most polite of essay collections.

Revealing is the fact that the goal posited for Japanese blue-collarites is the goal of ceasing to worry about the toil of work, forgetting about status comparisons, and becoming a "true middle-class man in an industrial society." Echoed elsewhere in other essays also, is a sound uneasily like the 1950s and the Human Relations Movement: this erstwhile goal of

shabby accommodation falls far short of the self-actualization that we "intellectuals" posit for ourselves, and blithely overlook when prescribing gratuitously for others (especially blue-collarites).

In this connection another mannerism of ours disappoints me, as the book is bereft of any revealing, hard-hitting, and constructive criticism of the thinking of one's colleagues save, perhaps, for Otto Neuloh's piece on German sociological disputations. Instead, author after author neatly capsulizes the already well-known works of major colleagues, and neatly arranges these in a predictable and colorless catolog. Little effort is made to identify contradictions, elements of confusion, or other critical takeoff points. Everyone is very nice to everyone else, and every essay loses much, therefore.

This is not to say the book is without merit. If only for its scope (a dozen or more locales), its several hundred academic references, and its broad range (the elitism of Japanese blue-collarites, the primitiveness of new industrial migrants) the book warrants purchase and cautious use. Ironically, however, its greatest value may lie in its omissions: The book points up by indirection what richer pathways we had better soon pursue if we are to ever infuse the study of blue-collarites with flesh, mind, and soul.

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Decisions by Juvenile Officers[1]

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This paper examines decision making by juvenile officers, looking at the effect that race and socioeconomic status of the individual, and racial composition and socioeconomic status of his neighborhood, have on juvenile police disposition. Data from Washington, D.C., indicate little, if any, influence of any of these factors. An analysis of data from Syracuse, N.Y., using a variation of Blau's structural effects technique, likewise indicate no effect of any kind. It is suggested that the juvenile officer's discretion may be viewed as an example of enacted institutional change.

Many recent studies indicate that the police develop priorities and establish cues to signify danger, impropriety, or suspicion (cf. Banton 1964; LaFave 1965; Skolnick 1966; Wilson 1968). Wilson neatly summarizes the literature when he writes: "The patrolman believes with considerable justification that teenagers, Negroes, and lower-income persons commit a disproportionate share of all reported crimes; being in those population categories at all makes one statistically more suspect than other persons" (p. 40).

This situation seems to be somewhat more acute when the police are dealing with juveniles. Werthman and Piliavin (1967, p. 72) note that "the juvenile officer exercises a good deal of discretion in deciding how to process offenders, a discretion that far transcends the measure of ambiguity ordinarily involved in legal assessments of motivation and intent."

Studies of juveniles have shown that, on the whole, black delinquents are younger than white delinquents at the time of their first court appearance and detention (Axelrad 1952); black youth are more often referred to court for less serious offenses than white youth (Axelrad 1952; Goldman 1963); black youth are more likely to be stopped and questioned than white youth (Piliavin and Briar 1964); and black youth have a higher arrest rate than white youth (Black and Reiss 1970). However, Black and Reiss (1970, p. 76) feel that "evidence that the police behaviorally orient themselves to race as such is absent." This position is supported by Skolnick (1966), Green (1970), Goldman (1963), and Cicourel (1968). They have suggested that the police do not behave differentially toward race; rather, the police react to race merely as an indicator of socioeconomic status.

[1] The authors would like to thank Howard Taylor and Mark Abrahamson for their many helpful comments on an earlier version of this paper.

The important fact to be noted in the previous studies is that they generally concern themselves with the interaction between juveniles and the beat patrolman (with the exception of Cicourel 1968). However, in many U.S. cities the beat patrolman who arrests a juvenile brings him to a specialized youth bureau. While the decision to arrest still lies with the patrolman, the disposition decision now lies with the juvenile officer: he decides whether to dismiss the youth, to send him to court, or to refer him to some social agency.

The purpose of this study is to examine the behavior of juvenile officers. The literature indicates that the decisions of patrolmen are influenced by a youth's race and socioeconomic status. As police departments throughout the country increasingly develop specialized youth bureaus, it is important to study how they handle juveniles and whether there is differential treatment by race and socioeconomic status at the youth bureau level.

The study was conducted in Washington, D.C., and Syracuse, N.Y. The two cities provided useful contrasts. Washington is a fairly large-sized city, with a population of 763,956 (according to the 1960 census), while Syracuse is a small city, with a population of 216,038. Washington has a majority of blacks and other nonwhites (55 percent), while Syracuse has proportionately few blacks and other nonwhites (6 percent). However, there are so few nonwhites other than Negroes in our study areas that we will refer to nonwhite areas simply as black. Of course, both cities have youth bureaus.

WASHINGTON, D C.

The initial study area was Washington. The basic unit of analysis was the census tract. Analysis of the association, if any, between the ratio of court referral to police contact rates and economic status, and this ratio and race, was determined by computing Pearsonian correlation coefficients for 109–13 of the 125 census tracts in the city (the tracts eliminated were sparsely settled or consisted of institutional populations).

Data for Washington consisted of records of 6,099 youth contacted by police of the District of Columbia in the fiscal year 1963. The Management Office for the District and its statistician at that time, Mr. Al Minlin, devised a form for the Police Department on which all juvenile contacts were recorded and filed in the Youth Aid Division. Of these contacts, 2,300 were referred to Juvenile Court. The remainder were dismissed without official action.

From addresses listed on the contact forms, the juveniles were assigned to census tracts. All were classified by area of residence, except 282 youth whose addresses were unknown. It is our belief that these unknowns are not randomly distributed throughout the city but are concentrated in the

lowest socioeconomic area. Our reason for suspecting a concentration of these unknowns is an observed discrepancy in the distribution of court referral rates by socioeconomic area between this study and another study in Washington (Willie and Gershenovitz 1964).

In the other study, court-referred youth were combined for a thirty-three-month period (July 1959–March 1962), and rates were computed for the same set of socioeconomic areas used in this study. Rates varied from a low of 4.3 in Area I (high SES), to 16.1, 29.9, and 33.9 in Areas II, III, and IV, to a high of 63.6 in Area V. The current study is limited to data collected in fiscal year 1963, which is one year later than the final year included in the period of analysis for the earlier study. The distribution of rates for the current study are similar to those in the other study for all socioeconomic areas except Area V. Here, the rates vary from 4.8 in Area I, to 16.3, 25.8, and 31.8 in Areas II, III, and IV. However, Area V in this study has a rate of only 49.2 as compared with a rate of 63.6 in the earlier study, using data from a similar period. Our assumption is that the rate for the lowest socioeconomic area in this study would be higher and similar to the rate in the other study if the 282 unknowns were included. These data, although circumstantial, are part of a growing body of evidence that the assumption often made in the past—that unknowns are randomly distributed throughout the population—is probably unwarranted.

The 1960 census population ten through seventeen years of age was used as the base for computing rates. The base was limited to these particular age levels because less than 5 percent of court-referred youth in Washington were under ten years of age. Because these youth are not so vulnerable as those over ten years of age, their inclusion in the population base would have distorted the actual incidence of delinquency by depressing the rate.

The socioeconomic status of the population was measured by a composite index score for each census tract. Five highly correlated variables were used in the index (see Willie and Wagenfeld 1962): (1) percentage of the employed in the combined categories of operatives, service workers, and laborers; (2) median number of years of school completed by the population over twenty-five years of age; (3) estimated market value of owned homes; (4) the gross monthly rental for tenant-occupied dwellings; and (5) percentage of sound dwelling units. (The first factor was inverted so that it varied directly with the other four.) Distributions of each of the five variables were converted into standard scores with assigned means of 50 and assigned standard deviations of 10. The five standard scores for each census tract were simply averaged into one composite socioeconomic status score. The population distributions by area and median home value may be seen in table 1.

TABLE 1

POPULATION DISTRIBUTION AND AVERAGE INCOME OF SOCIOECONOMIC AREAS, WASHINGTON, D.C.

Socioeconomic Area	Percentage of Population in Area	Median Home Value
I	10.6	$25,000
II	24.5	17,200
III	19.6	13,600
IV	36.3	13,000
V	9.0	11,800

Census tracts were further classified as white or black for the social area analysis; tracts in which two out of every three persons were white were classified as part of the white area. A similar procedure was followed in delineating the black area. Ninety-five percent of the total population in the white area was of Caucasian racial stock, and 90 percent of the total population in the black area was Negro. Thus, most of the census tracts in Washington are racially homogeneous; only about one-fifth of the population lives in racially mixed residential areas.

TABLE 2

POLICE CONTACTS AND COURT REFERRALS PER 1,000 YOUTH, AGES TEN THROUGH SEVENTEEN; AND RATIO OF COURT REFERRAL RATE TO POLICE CONTACT RATE, BY SOCIOECONOMIC AREA, WASHINGTON, D.C., 1963

Socioeconomic Area	Police Contact Rate	Court Referral Rate	Ratio of Court Referral Rate to Police Contact Rate
I	14.8	4.8	0.32
II	48.8	16.3	0.33
III	70.5	25.8	0.37
IV	77.3	31.8	0.40
V	139.7	49.2	0.35
Total city	71.0	26.8*	0.37

* True city rate is 30.1 per 1,000 youth when computed without regard to residence. The computation in this table excludes 282 youth whose addresses are unknown and therefore cannot be assigned to a socioeconomic area. The omission of the unknowns depresses the total city rate, which is based on all census tracts combined, and possibly also the rates in Area V.

The data in table 2 confirm the findings of many other studies and clearly indicate an inverse relationship between the distribution of juvenile delinquency and socioeconomic status. The lowest court referral and police contact rates are found in the area of highest socioeconomic status rank, and the highest rates are found in the area of lowest rank. For all areas, the police contact rate is approximately three times greater than the court referral rate.

Within the total city, 37 percent of all youth contacted by police are

referred to Juvenile Court (as seen in table 2). A regular pattern of increase in the proportion of young people referred to court is associated with a decrease in the socioeconomic status of the area of residence, from Areas I through IV. (The decrease in Area V below Area IV is probably an artifact due to the unknowns not tabulated, as mentioned above.) However, the variation in the ratios from 0.32 to 0.40 around a city-wide ratio of 0.37 is miniscule and probably not statistically significant. Pearsonian correlation coefficients confirm this assessment. Court referral–police contact ratios were computed for each census tract. These ratios were then correlated with socioeconomic status scores for 109 of the census tracts. The correlation coefficient of .06 for these two distributions is obviously not significantly different from zero. Thus, we conclude that socioeconomic status appears not to be a contributing influence to the juvenile officer's decision as to whether or not a youth contacted by the Washington, D.C., police is referred to Juvenile Court.

TABLE 3

POLICE CONTACTS AND COURT REFERALS PER 1,000 YOUTH, AGES TEN THROUGH SEVENTEEN; AND RATIO OF COURT REFERRAL RATE TO POLICE CONTACT RATE, BY RACIAL AREA, WASHINGTON, D C, 1963

Racial Area	Police Contact Rate	Court Referral Rate	Ratio of Court Referral Rate to Police Contact Rate
White	37.2	12.8	0 34
Black	84 6	32.7	0 38

The data in table 3 indicate differentials by racial area of residence in the rates of police contact and court referral. From two to three times more blacks than whites are contacted and referred to Juvenile Court (as seen in table 3). The important figure for this analysis, however, is the ratio of contacts and referrals, which helps us determine if there is discrimination in the handling of contacted black and white youth by professional police in the Youth Aid Division. While police in the field tend to have greater contact with black youth compared with white, the disposition process appears to be even-handed; the 38 percent of blacks referred to Juvenile Court is not very much greater than the 34 percent of whites who are referred to court. Moreover, a Pearsonian correlation coefficient was computed for a census tract distribution of ratio scores of police contacts and court referrals and the percentage of whites within the tract population. This resulted in an extremely low coefficient of −.16. On the basis of this analysis, we conclude that little difference exists in the court referral process for black and white youth in Washington, D.C.

Our general findings for Washington, then, are that the race and socioeconomic status of census tracts are related to the incidence of contact between police and youth. A higher proportion of youth in poorer neighborhoods and black residential areas come into contact with the police. The court referral process, however, appears to be reasonably fair, with similar proportions of the contacted affluent and poor youth as well as black and white youth being referred to Juvenile Court. In general, about one-third of all contacted youth are referred to court, and this proportion is descriptive of the experience of youth different racial and socioeconomic groups.

SYRACUSE, N.Y.

Because of the ecological fallacy (Robinson 1950), the data from Washington could not be compared satisfactorily with the previously cited studies of police and individuals. Further, data on individuals could not be obtained in Washington. Nonetheless, a comparison of police reaction to neighborhood characteristics with police reaction to the characteristics of individuals is clearly desirable. Therefore, a variation of structural effects analysis first articulated by Blau (1957, 1960) and Davis, Spaeth, and Huson (1961) was employed, and the influence of four variables—an individual's race and socioeconomic status, and the race and socioeconomic status of his neighborhood—on the disposition decisions of juvenile officers was examined.

Structural effects analysis permits us to make a comparison of individual and group data. According to Blau (1957), such a comparison consists of three steps: (1) obtain some measure of an individual trait; (2) combine the individual measures into one index for the group, thus creating another level of the same variable; and (3) (in order to isolate the effect of the group, the "structural effect") determine the relationship between the group attribute and some dependent variable, holding constant the individual attribute "The general principle is that if ego's X affects not only ego's Y but also alter's Y, a structural effect will be observed" (Blau 1957, p. 64).

To obtain the data for structural effects analysis, we made use of the Sheriff's Department Central Register in Onondaga County (Syracuse), New York. The Central Register, similar to the Youth Aid Division file in Washington, contains an information card on every juvenile (ages seven through fifteen) contacted by all police agencies in the county. The data consisted of the records of 1,351 youth contacted by the police in 1968. Of these, 946 were referred to Family Court, 389 were dismissed without official action, and sixteen were referred to some social agency.

The disposition decision of the juvenile officer was obtained directly

from the Register cards, as was the youth's race. This latter was classified as black or white, since other nonwhites (American Indian and Oriental) comprised only 5 percent of the base population.

From addresses listed on the cards, father's occupation was obtained using the city and suburban directories. As a measure of individual socioeconomic status, each occupation was then assigned to one of the seven categories in Hollingshead's (1957) two-factor index of social position. Neighborhood racial composition and socioeconomic status were determined as in Washington, using the census tract as the unit of analysis. There are sixty-one census tracts in the city of Syracuse and an additional twenty-nine in the remainder of Onondaga County. As in Washington, the tracts are racially and socioeconomically homogeneous, with only two tracts out of ninety showing a high degree of heterogeneity.

In order to determine the reliability of our Washington findings, we used four measures of census tract socioeconomic status: (1) the five-factor index employed in the Washington data; (2) the percentage of professionals in each tract (Hollingshead's classes 1 and 2); (3) the percentage of semi-skilled and unskilled workers in each tract (Hollingshead's classes 6 and 7); and (4) the ratio of the percentage of professional to the percentage of semi-skilled and unskilled in each tract.

(It should briefly be noted that there are well-known, potential dangers in using official statistics. However, here we are using official statistics in the manner suggested by Kitsuse and Cicourel [1963]: to explain how individuals *come to be defined as deviant by the organization.*)

The structural effects technique used here differs from conventional structural effects analysis in two respects. First, Blau employs structural analysis to examine the effect of ego's trait and ego's group trait *on ego.* However, we have focused on the effect of ego's trait and ego's group trait, not on ego, but on *alter.* In other words, we have examined the effects that the race and socioeconomic status of a youth and his neighborhood have, not on himself, but on the disposition decisions of the juvenile officer.

It will be recalled that Blau determines the group trait by combining the individual traits into one index for the group. This makes his so-called group trait nothing more than another level of the same variable. While we too have done this, we have also employed a measure of group socioeconomic status (the five-factor index used on the Washington data) that is largely independent of the individual trait (occupation). This may be viewed as an attempt to bring us closer to a true separation of individual and group traits, as sought by Blau.

The individual unit of analysis was the individual youth and not the offense committed. Therefore, it was necessary to devise some single measure that would indicate the individual's disposition history. To do

this, each youth was assigned a disposition score, computed as follows: an individual received a score of + 1 each time he was referred to court, a score of − 1 each time he was dismissed, and the two scores were then summed to give a relative disposition score. Thus, a youth with a score of + 2 was sent to court two times more than he was dismissed. It is possible for such a score to be ambiguous; it may mean that a person was either contacted by the police twice and referred to court twice, or was contacted four times, referred to court three times, and dismissed once. However, the possible ambiguity never seriously presented itself in our data: 80 percent of all youth contacted by the police were contacted only once.

After obtaining the necessary data, we performed two-way analysis of variance. It may be seen from table 4 that there is an interaction effect

TABLE 4

TWO-WAY ANALYSIS OF VARIANCE· INDIVIDUAL SES, PERCENTAGE OF SEMI-SKILLED AND UNSKILLED IN TRACT, AND DISPOSITION SCORE

	Percentage of Semi-skilled and Unskilled in Tract					
Individual SES	Low % Semi-skilled and Unskilled (High SES)	2	3	4	5	High % (Low SES)
Classes 1–4 (high SES): $\overline{X}$ =	0 5198	0.4957	0.6349	0.5699	1.2317	0 7407
Classes 5–7 (low SES)· $\overline{X}$ =	0 6108	0.5605	0 6835	0.5351	0.5703	1 0877

NOTE.—Individual $F = 1\ 40$, $p > 05$ (N S); tract $F = 1\ 11$, $p > 05$ (N S); interaction $F = 2\ 50$, $p < 05$

of individual socioeconomic status and tract socioeconomic status (measured by the percentage of semi-skilled and unskilled in the census tract) on the disposition a youth receives from juvenile officers. The highest disposition score (1.2317) is found among youth of high individual socioeconomic status but low tract status. Obviously, then, the police refer to court a high percentage of high-status youth who live in low-status neighborhoods, possibly in an effort to "protect" them from their environment. Table 4 also shows, not surprisingly, that the group next most frequently referred to court are low-status youth in low-status tracts.

In spite of these findings, the table indicates no significant individual effect and no structural effect. That is, the socioeconomic status of the individual youth and the socioeconomic status of the neighborhood in which he lives do not appear to affect the disposition decision of the juvenile officer.

Table 5 shows both an individual effect and a structural effect, but no interaction effect. This indicates that youth with low individual socioeconomic status are referred to court more frequently than youth with

TABLE 5

TWO-WAY ANALYSIS OF VARIANCE. INDIVIDUAL SES, PERCENTAGE OF PROFESSIONAL IN TRACT, AND DISPOSITION SCORE

	Percentage of Professional in Tract					
Individual SES	Low % Professional (Low SES)	2	3	4	5	High % (High SES)
Classes 1–4 (high SES): $\overline{X} =$	1 3333	0 6794	0 8667	0.2190	0 2792	0.4135
Classes 5–7 (low SES): $\overline{X} =$	1.2360	0 6031	0 6259	0 4880	0 6589	0 5223

NOTE —Individual $F = 2\ 29$, $p < .05$; tract $F = 3\ 52$, $p <\ 05$, interaction $F = 1\ 45$, $p >\ 05$ (N S)

high status, as might be expected. Further, youth from high-status neighborhoods, measured here by the percentage of professional in the tract, are referred to court less frequently than youth from low-status neighborhoods. That is, the higher the tract status, the lower the court referral rate. While the decreasing sequence of scores is not consistent, apparently the decrease is statistically significant.

In table 6, another structural effect may be observed; this time tract

TABLE 6

TWO-WAY ANALYSIS OF VARIANCE INDIVIDUAL SES, RATIO OF PERCENTAGE OF PROFESSIONAL TO PERCENTAGE OF SEMI-SKILLED AND UNSKILLED IN TRACT, AND DISPOSITION SCORE

	Ratio of Percentage of Professional to Percentage of Operative				
Individual SES	0 0–0 18 (Low SES)	0 20–0 38	0 39–0 57	0 59–1 05	1 06–5 77 (High SES)
Classes 1–4 (high SES): $\overline{X}$ =	0 9444	1 2429	0.1937	0 1889	0 2867
Classes 5–7 (low SES)· $\overline{X}$ =	1 1326	0 5111	0 4980	0 8333	0 4810

NOTE.—Individual $F = 1\ 78$, $p >\ 05$ (N S), tract $F = 2\ 56$, $p <\ 05$, interaction $F = 1\ 45$, $p >\ 05$ (N S)

status is measured by the ratio of percentage of professional to percentage of semi-skilled and unskilled in the tract. There is a general decrease in the disposition scores, from 0.9444 to 0.2867 and from 1.1326 to 0.4180, as the ratio increases. Again, the status of the youth's area of residence affects the disposition decision of the juvenile officer: as status increases, court referral decreases. However, table 6 shows that there is no individual effect of status and no interaction effect.

Finally, table 7, with tract socioeconomic status measured by the same five-factor index employed in the Washington data, indicates no effect of any kind: no structural effect of census tract socioeconomic status, no individual effect of socioeconomic status, and no interaction effect of the two.

TABLE 7

TWO-WAY ANALYSIS OF VARIANCE: INDIVIDUAL SES, FIVE-FACTOR INDEX, AND DISPOSITION SCORE

INDIVIDUAL SES	CLASS: High SES	2	3	4	Low SES
Classes 1–4 (high SES): $\overline{X}$ =	1.0000	0.4177	0 2988	0 7036	0 3788
Classes 5–7 (low SES): $\overline{X}$ =	0.5259	0 4754	0 4090	0 5432	0 9810

NOTE —Individual $F = 1\ 65$, $p > 05$ (N S), tract $F = 21$, $p > 05$ (N S), interaction $F = 1\ 43$, $p > 05$ (N S).

To summarize the findings from the Syracuse socioeconomic data, we have found one individual effect, two structural effects, and one interaction effect. Since it was possible to have four effects of each type (the structural effect of socioeconomic status as measured by the five factor index, percentage of professional in the tract, percentage of semi-skilled and unskilled in the tract, and the ratio of percentage of professional to percentage of semi-skilled and unskilled; and a corresponding individual effect and interaction effect for each), several conclusions may be reached. First, with only one individual effect out of four, the socioeconomic status of the individual youth may be said not to affect the disposition decisions of juvenile officers. Second, there appears to be little interaction between individual status and tract status in influencing disposition decisions, as indicated by the presence of only one effect out of four. Finally, with two structural effects out of four, the influence of census tract socioeconomic status on disposition decisions may be questioned, but this is definitely the strongest influence.

Examining the Syracuse data on race (table 8), it may be seen that there is no significant effect of race on disposition decisions: no individual effect of race, no structural effect of tract racial composition, and no interaction effect of the two. Quite the contrary to what we might imagine, the race of an individual youth has no influence on the disposition deci-

TABLE 8

TWO-WAY ANALYSIS OF VARIANCE· INDIVIDUAL RACE, TRACT RACIAL COMPOSITION, AND DISPOSITION SCORE

INDIVIDUAL RACE	TRACT RACIAL COMPOSITION: Low % Nonwhite	2	3	4	High % Nonwhite
White: $\overline{X}$ =	0.4732	0.7301	0.6036	0 6119	0.8043
Black: $\overline{X}$ =	0.6667	0.8889	0.4737	0.9231	0.9249

NOTE.—Individual $F = .64$, $p > 05$ (N S); tract $F = 64$, $p > 05$ (N S), interaction $F = 20$, $p > 05$ (N S).

sions of the juvenile officer, nor does the race of his neighborhood, nor does an interaction of the two. In light of the emphasis in the sociological literature on racial determinants of police decision making, we feel that these findings are quite extraordinary. They indicate that, unlike the cop-on-the-beat, who has been shown to be biased toward blacks and the poor, the professional juvenile officer is apparently unbiased.

CONCLUSIONS

How can these findings be accounted for? How can we explain the lack of a strong relationship between race and socioeconomic status and the disposition decisions of juvenile officers, when the sociological literature clearly indicates that the cop-on-the-beat is biased?

There are several possible explanations. First, it is possible that juvenile officers are specially selected for their sympathy toward juveniles. However, we found that, while the juvenile officers *are* specially selected by their captain, rather than drawn at random or accepted as volunteers, the term "sympathy" was specifically rejected by the supervising officer. What is stressed, according to the captain, is "fairness to the juvenile." Based on interviews with policemen at all pay levels, however, we have reservations about the suggestion that the juvenile officers are *selected* for their fairness.

Thus, we can find nothing inherent in the juvenile officer's experience or training to explain this absence of bias in decision making with reference to racial and socioeconomic status characteristics of youth. Black and Reiss (1970) suggest that the organization (the Police Department, courts, and the juvenile justice system) expects different behavior from the juvenile officer compared with the cop-on-the-beat. Juvenile officers are not encouraged to make a "good pinch," as is the cop-on-the-beat. Juvenile officers are expected to be fair. This is an example of what Merton (1957) has called "enacted institutional change." He says that the only way to put a stop to such phenomena as anti-black and anti-poor bias is through "a deliberate and planned halt . . . under appropriate institutional and administrative conditions" (p. 435). Such a condition is the official sanction of the norm of fairness. Our belief is that this norm develops in those police departments which have specialized youth bureaus, and our findings suggest that, in terms of the behavior of its personnel, the organizational expectancies for such bureaus are more important than the kinds of individuals who staff them.

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Race and Ethnicity Relative to Other Factors in Juvenile Court Dispositions[1]

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Research up to this point has been inconclusive about whether the overrepresentation of minority-group members in our official correctional statistics is mostly a function of artifacts of data collection, the nature and volume of crimes committed by persons in these categories, or bias in the correctional process. This paper examines the data from the records of a juvenile court in a middle-sized city. The likelihood that the probation officers will submit more cases from minority groups than from Anglos for formal court hearings is reduced below levels of statistical significance when appropriate considerations are taken into account—except for those for whom extensive handling seems most logical. The likelihood that the judge will send more minority-group members than Anglos to the youth authority, however, persists for most categories of offenders even when these considerations are taken into account. The biases appear to consist largely of letting Anglos "get off easy" rather than of requiring uncalled-for "treatment" for minority-group members. Overall, the findings suggest that about two-thirds of the differential handling in the court studied is not explained by appropriate considerations.

Attitudes of the majority group toward racial minorities influence most aspects of the lives of the minority-group members, including, presumably, the social control measures applied to them. The center of attention here is criminal deviation. The social control mechanisms of central concern are two decisions made within the structure of the juvenile court—the decision by the probation officers to submit cases to the courts for formal hearing and the decision by the judge to commit individuals to the state youth authority which, in turn, places most of those it receives in training schools. The question is whether these decisions are biased against persons in minority groups. It is altogether possible, of course, that differential handling of Anglos and the other groups by the police may eliminate Anglos disproportionately from coming to the attention of the juvenile court, but that is not the problem for this paper.

[1] This paper was read to the Crime and Delinquency Committee of the Society for the Study of Social Problems, Miami, Florida, August 1966. The research was carried out under mental health small grant MH 1103301. The author appreciates the comments and suggestions made by Norval D. Glenn and J. Allen Williams about an earlier version of this paper.

At the present time we do not know whether decisions about handling offenders are characterized by racial bias. There is no doubt, however, that lower-class and minority-group persons are overrepresented at every stage of our correctional process (Short and Nye 1957, pp. 208–09, and a long tradition of delinquency-area research; Sellin 1935; Korn and McKorkle 1959, pp. 336–43; Bloch and Geiss 1962, p. 420; Elliot 1952, p. 561; Sutherland and Cressey 1966, pp. 145–52). The issue is the extent to which this overrepresentation can be explained by each of the following sets of variables.

Artifacts of data collection.—Most of these artifacts would seem to raise the Negro crime rate artifically. Negroes are concentrated in those ages most likely to be criminal (Johnson 1964, p. 82). This population is also concentrated in northeastern cities in which our reporting is at its best (Johnson 1964, p. 83). This concentration means that urban lower-class Negro deviance is more likely to come to the attention of correctional agencies than is the deviance of either Negroes in the rural South (where the extended family structure can handle many of the problems of individuals) or the dominant population (which more commonly turns to noncorrectional agencies such as counselors of various types) (Johnson 1964, pp. 82–83; Bloch and Geiss 1962, pp. 420–21). The "professionalism" of the police and others who handle juveniles appears to be a variable in the bias in his handling (Wilson 1967), but its overall effect is not clear. It appears to counteract the effect of population concentration, making Negro and white rates more equal in the areas where the better reporting creates the overall impression of higher Negro crime rates.

Characteristics of the offenders and their offenses which justify more extensive handling of minority-group members.—The overrepresentation of lower-class, minority-group persons in the correctional process is, most directly, a culmination of high conviction, imprisonment, and retention rates of persons in these categories. These rates, some have argued, result from the fact that "the crimes for which Negroes are sentenced tend to include a higher proportion of those felonies for which prison sentences are ordinarily longer" (Korn and McKorkle 1959, p. 239; see also Johnson 1964, p. 82), for which the percentage of offenses cleared by arrest is high, and for which release into the community is least feasible. Minority-group members may also appear to be less tractable, more defiant, and, hence, more likely to be recidivists if left in the community (Piliavin and Briar 1964, pp. 212–13).[2] Differentials in handling resulting from slight differences in types of offenses committed or attitudes displayed could accumulate over a period of time into a marked overrepresentation of

[2] This finding was that poor "demeanor" resulted in the police taking further action relative to the juvenile. The observation that poor demeanor was associated with minority status is a relatively minor point for these researchers.

minority-group members in the correctional system. Such considerations about the nature of the data about criminals permitted Moses to conclude that there was "no evidence of a policy whereby conviction rates were unfavorable to Negroes" (1947, p 419).

The race and class bias of those responsible for persons at various stages in the correctional process.—Differences which strongly imply discrimination have been observed in the decisions by police about disposition of juveniles (Piliavin and Briar 1964, pp. 212–13), in the composition of juries (Robinson 1950), in provision of counsel to defendants (Sutherland and Cressey 1966, pp. 379–80), in commitments to prison (Korn and McKorkle 1959, pp. 236–38), and in length of sentence (Korn and McKorkle 1959, p. 239).[3] Axelrad (1952, pp. 569–74, quoted in Johnson 1964, p. 83), looking at a training-school population to see the accumulated results of these differentials, found Negro inmates to be younger on the average than whites, "to have been institutionalized after a smaller number of court appearances, to have had less exposure to probation, and to have had more of their numbers committed for less serious offenses."

Determining the relative importance of these three explanations of the overrepresentation of certain categories of persons in official data is probably most difficult when dealing with data on juveniles.[4] The philosophy of the juvenile court (which has tended to become the philosophy of the entire correctional process for juveniles) is that the action of all relevant officials is to be in the best interests of the child and the community. Thus, the discretion left to these officials is greater than that for most correctional officials. Accordingly, there are all sorts of variations in record keeping practiced in various jurisdictions, and there are a number of perfectly "appropriate" considerations in handling offenders within which race or class discrimination could be hidden. These considerations cover the whole range of economic and social "factors" associated with the causes of delinquency and recidivism.

HYPOTHESIS

The general purpose of this paper is to determine the relative importance of the second set of variables (characteristics of offenders and their offenses) and the third set of variables (race and class bias of those responsible for making decisions about the disposition of the juveniles) in the

[3] This difference is even greater when the effect of the race of the victim is taken into account (Johnson 1944) For additional references, see Sutherland and Cressey (1966, p 146, n 4).

[4] Overrepresentation here means that these groups are represented in correctional data more often than they are in the population at large See, among other references, Cloward and Ohlin (1960, p 39).

disposition of offenses by one court. Unfortunately, the records on which this research is based do not fully describe all of the characteristics of the offenders which may be considered appropriate grounds for differential disposition. Most notably lacking is information about the attitudes that the offenders (and/or their parents) may have displayed during the investigations or hearings. In operational terms, then, the hypothesis is that simultaneous *statistical control for the marital status of the juveniles' parents, the seriousness of the offense, the number and seriousness of prior and concurrent offenses, and the delinquency rates in the offenders' neighborhoods will reduce below statistical significance the apparent bias in the data against persons of minority-group status.*[5]

METHODOLOGY

The general procedure here is to minimize the first set of variables which are thought to have an effect on the general overrepresentation of minority-group members in official crime data by studying only one community and to determine the significance of the second class of variables (characteristics of offenders and offenses) when race and ethnicity are held constant. Holding the first set of variables relatively constant, however, means that results based on one community cannot be indiscriminately applied to other communities. While the community studied is in the South, it is not in the Deep South and has many characteristics "typical" of many middle-sized American communities of about 250,000 population. No claim is made, however, that the court's cases or operations are truly "representative." For example, the operation of the Crime Prevention Bureau (juvenile squad) of the police department may not be typical. Indeed, observation of this bureau by one of my students indicates that most offenders referred to the court either are insolent to the police or are repeated offenders. Since the personnel of this bureau are aware of the possible effects their actions may have on future job opportunities of the youths they handle, few middle-class individuals are referred to the court.

The data used here come from the files of the juvenile courts in Lake City. The files on cases receiving "brief service"—only one or two interviews with a probation officer—are, of course, less complete than those containing social investigation reports—cases for which files on the family are established. In the latter cases, all information on members of the family is placed in its file folder. These files are thorough, orderly, and constantly revised. For reasons relative to a larger research project, the individuals chosen for study were born between September 1, 1947 and

[5] In other words, these considerations are taken as independent variables, all of which are hypothesized as having a part in the production of the dependent variable of the results of court action

December 31, 1948. Each offense committed by these individuals and made a matter of formal or informal record in the court prior to April 9, 1964 was analyzed by assistants who read and coded the data.[6]

In order to relate disposition of a case by the court to seriousness of offense, offenses were classified by seriousness as shown in table 1. The

TABLE 1

CLASSIFICATION AND FREQUENCY DISTRIBUTION OF OFFENSES

Seriousness Level 1 (Least Serious)	Seriousness Level 2	Seriousness Level 3	Seriousness Level 4 (Most Serious)
Profane language on phone (3)	Drunk in public place (5)	Theft, motor vehicle (44)	Assault with intent to maim (20)
Abusive language (4)	Drunk juvenile (8)	Misdemeanor and felony theft (over $5.00) (266)	Assault (14)
Illegal possession of alcohol (8)	Vandalism (63)	Robbery (including armed robbery) (8)	Murder (1)
Minor in possession of illegal beverage (6)	Runaway (134)	Passing and forging checks (8)	Negligent homicide (1)
Petty theft (under $5 00 value) (86)	Carrying prohibited weapon (5)	Threatening to do bodily harm (1)	Sex offenses (16)
Traffic offenses (4)	Vagrancy (3)	Altering and passing U S coins (2)	
Truancy (23)	Car prowling (2)		
Indecent proposal (1)	Resisting arrest (1)		
Disturbing the peace (9)			

NOTE--Figures in parentheses are number of cases

classification was made on grounds which were largely intuitive, but probably reflect a reasonable approximation of the feelings of middle-class persons who define morality.[7] Development of some more objective ranking of the seriousness of offenses would constitute another research project

[6] Grateful acknowledgement must be made to Wesley S Calverley who was in charge of case analysis, Peggy Shulte Patterson and Vincent Hirsch who assisted in case analysis, Gerry Blaylock who made an initial analysis of the data, and Davie Louise Chitwood who assisted with data tabulation.

[7] It is impossible to use juvenile-court laws to define which acts are more serious than others The philosophy of the court clearly implies that a given act will be considered more serious in one case than another. In addition, the juvenile-court laws usually include far-reaching clauses concerning behavior that is detrimental to the juvenile or others. The law under which this court operated is typical in these respects.

The Sellin and Wolfgang ranking system (1964, pp. 298–300) that was developed in Philadelphia is exceedingly difficult to apply to the present data, which are from a juvenile court rather than the police; they provide very little of the detail needed to score "events" by the Sellin and Wolfgang methods. Further, twelve of the twenty-eight offenses listed in table 1 are offenses without victims and, hence, not in those "certain kinds of delinquency events which will yield the data that can be used in the construction of an index" (Sellin and Wolfgang 1964, p. 300) based on injury or damage to society. Sellin and Wolfgang note (p. 301) that one-third of all delinquency events have to be eliminated in their scoring system.

Race, ethnicity, and occupation of head of offenders' households were taken from the court records; unemployed heads of households were classed according to their last known occupation. (If the last job did not appear on the records or if the family's principal support was from welfare funds or the AFDC program, the family was placed in the lowest category of occupations used by the Census Bureau.) "Middle" rank includes all those heads of households who were "operatives or kindred workers" or higher. Children from homes headed by workers in "private households" or "service . . . except household" were taken as a second social rank. Children of "laborers, except farm and mine" were taken as the third rank. These classes bear no precise correspondence to better-known schemes of stratification but will be called "middle," "upper lower," and "lower lower ranks" in this paper.

The measures of considerations hypothesized to account for the apparent bias of the court are:

1. Marital status of juveniles' parents: broken or intact home, based on information in court files; broken home was scored a weight of 3 when combined with other variables.

2. Seriousness of the offense: the classification appearing in table 1.[8]

[8] A typographical error on the coding sheet caused the combining of two of the three categories of theft Misdemeanor and felony theft were combined into one category consisting of all thefts of items worth more than $5 00, while petty theft (theft of items worth less than $5 00) remained in a separate category The error resulted in moving an undetermined number of offenses from the "seriousness" level 2 to 3 There are, however, three reasons for believing that the error had a relatively small effect on the results. (*a*) Because there are four categories of seriousness, the errors do not represent a full change of direction. Further, by assigning closely similar numerical values (1, 2, 3, and 4) to the seriousness levels, the weight of the error is relatively small in the combined results appearing in table 9. (*b*) The findings suggest that the practices of the court with regard to the "worst" offenses differ from those relative to other offenses. It is probable that the practices of the court relative to misdemeanor theft are similar to those for other seriousness level 2 offenses. Taking data about *seriousness level 2 offenses out of the data on more-serious offenses would presumably exaggerate* the differences already found between more-serious offenses and other offenses rather than change the present findings. (c) The effects of this error on the

3. Number and seriousness of prior and concurrent offenses: only offenses known to the court were used, and they were assigned weights ranging from 1 for the least-serious to 4 for the most-serious offenses. "Prior offense experience," "concurrent offense experience," and "total offense experience" scores were computed. Each offense in a multiple charge was given the same weight as each known prior offense, since both classes of offenses are equally a part of the experience of a juvenile.

4. The delinquency rate in the offenders' neighborhoods: total number of offenses[9] known by the police to have been committed in each census tract by fifteen-year-olds in 1963 and sixteen-year-olds in 1964 (i e., by individuals approximately the same ages as those whose court records were studied) divided by the number of eleven- and twelve-year-olds in each census tract in 1960. The twenty-four census tracts in Lake City were classified into upper, middle, and lower thirds by rates of offenses thus derived. When various items were combined, the high delinquency tracts were given a weight of 3, the middle rate tracts a weight of 1, and the lowest rate tracts 0 weight. Obviously, these items may be combined in various ways by using the weights indicated.

Data relative to 794 offenses were examined. Sufficient information for analysis was provided for only 761. The two offenses by Orientals and the one offense by an American Indian are eliminated, so the N for all tables is 758.

FINDINGS

1. Minority-group members are more likely to have their offenses brought before the juvenile-court judge and more likely to be committed to the state youth authority than are majority-group members. Table 2, with the differential disposition by race and ethnicity when no auxiliary considerations are taken into account, reveals clear differences between the handling of white Anglos and of each of the two minority groups (see test of significance for difference of proportions, Hagood and Price 1952, pp. 315–18). Negroes are more likely to have their offenses heard before the

results were checked by the development of distributions for nontheft-offense data only. The changes in the patterns of distribution were almost negligible For example, the association between severity of disposition and social ranking remained positive for Anglos and Negroes and negative for Latin Americans after all thefts were removed from the data. The task of reviewing all the court records to correct the error would involve costs that seem prohibitive in view of the probable gain in precision

[9] Urban renewal projects and rapid development on some edges but not others of Lake City between the most recent census (1960) and 1964 made analysis by rates slightly inaccurate. However, the age structure and population of the census tracts varied enough to call for computation of rates.

TABLE 2

DISPOSITION OF OFFENSES BY RACE AND ETHNICITY OF OFFENDER (%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court	14 (331)	22* (224)	29* (203)
Decision 2: Judge sends to youth authority	24 (45)	71* (49)	71* (58)

NOTE.—Bases for percentages appear in parentheses.
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than .95.

court and to be sent to the state youth authority than are Latin Americans, and the difference between the probability of the two groups having their cases sent to court is significant. Data for this court reveal the differentials by race and ethnicity found previously to be characteristics of American correctional data.

2. Very little of the differential disposition of race and ethnic groups can be attributed to differences in disposition of persons by social ranks. The court records indicate that handling of persons in this court does not vary systematically by their social rank. Table 3 presents the relevant data

TABLE 3

DISPOSITION OF OFFENSES BY SOCIOECONOMIC RANK OF OFFENDER (%)

	Middle Rank (Down through Operatives and Kindred Workers)	Upper Lower Rank (Private Household and Service Workers)	Lower Lower Rank (Laborers)
Decision 1: Probation officer sends to court	35 (160)	29 (92)	32 (132)
Decision 2: Judge sends to youth authority	57 (56)	37 (27)	76* (42)

NOTE.—Bases for percentages appear in parentheses. Number undefined on socioeconomic rank = 375.
* Probability that persons of lower rank are more likely to be sent to the youth authority than are persons of middle rank is greater than .95.

for all the offenses taken together. It appears that in Lake City, at least, most of the overrepresentation of lower-rank juveniles is introduced into correctional data before the offenders come to the attention of the court and is not markedly changed by the court.

3. Characteristics of offenders and their offenses account for much of the differential disposition by race and ethnicity.

a) Marital status of offenders' parents is viewed by juvenile-court per-

sonnel as an index of the capability of the parents for exercising future control of the juveniles. Court personnel are aware that the "goodness" of a home is not necessarily attested by the presence of both parents, but if both parents appear to confer with the probation officer or judge, the home will likely be judged stronger than if one parent explains that the other parent is not in the home. Although marital status of offenders' parents ap-

TABLE 4

DISPOSITION OF OFFENSES BY RACE, ETHNICITY, AND MARITAL CONDITION OF OFFENDERS' PARENTS (%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court			
Home intact	13 (160)	19 (119)	25* (77)
Home broken	18 (126)	28* (74)	35* (98)
Decision 2: Judge sends to youth authority.			
Home intact	30 (21)	69* (23)	89* (19)
Home broken	19 (23)	81* (21)	59* (34)

NOTE.—Bases for percentages appear in parentheses Number defined on home status = 104
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than 95

parently does have an effect on the disposition of offenders in the Lake City court, holding marital status constant across the race and ethnic categories reduces relatively few of the differences among these categories below statistical significance.[10]

b) The seriousness of the offense is likely to be considered in the disposition of cases by the juvenile court. Although, strictly speaking, the offense is supposed to be subordinated to the needs of the juvenile,[11] dispositions often reflect community attitudes: those who have committed more serious offenses are more often seen to be in need of "treatment." Apparently the court in Lake City adheres to the philosophy of not making

[10] Parenthetically, however, it may be noted that these data illustrate the gradual accumulation of skewed distributions of demographic characteristics in the delinquent population Forty-four percent of the offenses brought to the attention of the court by the police and other agencies were committed by individuals from broken homes Fifty-six percent of the offenses for which this court sent individuals on to the state youth authority were committed by persons from broken homes The police and other "front-line" agencies tended to "overselect" those from broken homes; the juvenile court continued this overselection. It is not surprising that the population of the training schools, the final major stage in the correctional process, is markedly different from the population that admits to having committed delinquent acts (Short and Nye 1957, p. 210).

[11] As Albert Cohen pointed out to Short and Nye (1957, p. 209), equal handling of legally identical cases is "the negation of the court (and police) philosophy"

TABLE 5

Disposition of Offenses by Race, Ethnicity, and Seriousness of Offense (%)

	Anglo	Latin American	Negro
Decision 1 Probation officer sends to court:			
Seriousness level 1 (least serious)	10 (59)	8 (52)	16 (38)
Seriousness level 2	15 (113)	17 (64)	19 (48)
Seriousness level 3	14 (135)	32* (96)	35* (93)
Seriousness level 4 (most serious)	13 (24)	25 (12)	42* (24)
Decision 2. Judge sends to youth authority:			
Seriousness level 1 (least serious)	33 (6)	75 (4)	100* (6)
Seriousness level 2	18 (17)	64* (11)	89* (9)
Seriousness level 3	32 (19)	74* (31)	57* (33)
Seriousness level 4 (most serious)	0 (3)	67 (3)	80* (10)

Note.—Bases for percentages appear in parentheses.
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than .95.

seriousness of offense the predominant criterion for disposition. Taking seriousness of offense into account, however, does reduce a number of the differentials in disposition by race and ethnicity. The fact that this consideration does not eliminate all the differentials supports earlier findings that minority-group members are likely to receive greater punishment than whites for the same offenses. Table 5 also reveals a tendency that becomes more pronounced in succeeding tables—the concentration of the differential disposition among offenders with the "worst" records.

c) The number and seriousness of prior and concurrent offenses are viewed as appropriate considerations in juvenile-court dispositions. This record is taken to indicate whether the act under consideration is an isolated instance of misbehavior under unusual circumstances or an expression of a personality and/or situation which chronically produces delinquent behavior. As noted above, one of the chief criteria used by the Lake City police to determine whether to turn a juvenile over to the court is the extent of their prior contact with him. Consideration of the number and seriousness of prior and concurrent offenses markedly reduces the differential handling of juveniles of different races and ethnic groups. Table 6 indicates the extent of this reduction when simply considering the number of prior contacts with the court. Table 7 presents the dispositions when the seriousness of the current offense, the prior offense experience, and the concurrent offense experience are all held constant. The reduction of differentials in some categories means, of course, the concentration of the differential handling in other categories. Thus, Latin Americans and Negroes with extensive "records" are dealt with much more "extensively" (or harshly), on the average, than are Anglos with similar records.

d) The rate of delinquency in a neighborhood from which a juvenile

TABLE 6

DISPOSITION OF OFFENSES BY RACE, ETHNICITY, AND PRIOR OFFENSES KNOWN TO COURT
(%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court			
No prior offenses	10 (249)	14 (117)	16 (114)
One prior offense	28 (47)	28 (43)	27 (44)
Two or more prior offenses	23 (35)	33 (64)	62* (45)
Decision 2: Judge sends to youth authority.			
No prior offenses	21 (24)	50 (16)	22 (18)
One prior offense	23 (13)	83* (12)	83* (12)
Two or more prior offenses	37 (8)	81* (21)	96* (28)

NOTE: Bases for percentages appear in parentheses.
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than .95.

TABLE 7

DISPOSITION OF OFFENSES BY RACE, ETHNICITY, AND "TOTAL OFFENSE EXPERIENCE" ASSOCIATED WITH EACH OFFENSE
(%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court.			
Total experience ranks 1 or 2	9 (115)	5 (61)	6 (47)
Total experience rank 3	11 (97)	12 (51)	9 (46)
Total experience rank 4, 5, or 6	19 (72)	30 (43)	27 (48)
Total experience ranks 7 or more	21 (47)	40* (67)	61* (62)
Decision 2: Judge sends to youth authority			
Total experience ranks 1 or 2	20 (10)	67 (3)	67 (3)
Total experience rank 3	36 (11)	17 (6)	50 (4)
Total experience ranks 4, 5, or 6	14 (14)	69* (13)	77* (13)
Total experience ranks 4, 5, or 6	30 (10)	85* (27)	71* (38)

NOTE: Bases for percentages appear in parentheses.
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than .95.

comes may be seen as an index of the extent to which the juvenile's delinquency seems likely to continue because it is supported by his peers or by a neighborhood culture which commonly produces delinquent behavior. Thus, coming from a neighborhood in which there is a high delinquency rate becomes a partial justification for court personnel to perceive that a child is in need of "treatment." Consideration of neighborhood rates of delinquency reduces the differential disposition by race and ethnicity most noticeably for those tracts where the rate is low. The data were also analyzed by volume of delinquency in each census tract. This analysis produced a more consistent reduction in differential disposition by race and ethnicity than did any other consideration taken alone. It may be that volume of delinquency in different parts of town affects the court officials'

TABLE 5

DISPOSITION OF OFFENSES BY RACE, ETHNICITY, AND SERIOUSNESS OF OFFENSE (%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court:			
Seriousness level 1 (least serious)	10 (59)	8 (52)	16 (38)
Seriousness level 2	15 (113)	17 (64)	19 (48)
Seriousness level 3	14 (135)	32* (96)	35* (93)
Seriousness level 4 (most serious)	13 (24)	25 (12)	42* (24)
Decision 2. Judge sends to youth authority:			
Seriousness level 1 (least serious)	33 (6)	75 (4)	100* (6)
Seriousness level 2	18 (17)	64* (11)	89* (9)
Seriousness level 3	32 (19)	74* (31)	57* (33)
Seriousness level 4 (most serious)	0 (3)	67 (3)	80* (10)

NOTE.—Bases for percentages appear in parentheses.
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than .95.

seriousness of offense the predominant criterion for disposition. Taking seriousness of offense into account, however, does reduce a number of the differentials in disposition by race and ethnicity. The fact that this consideration does not eliminate all the differentials supports earlier findings that minority-group members are likely to receive greater punishment than whites for the same offenses. Table 5 also reveals a tendency that becomes more pronounced in succeeding tables—the concentration of the differential disposition among offenders with the "worst" records.

c) The number and seriousness of prior and concurrent offenses are viewed as appropriate considerations in juvenile-court dispositions. This record is taken to indicate whether the act under consideration is an isolated instance of misbehavior under unusual circumstances or an expression of a personality and/or situation which chronically produces delinquent behavior. As noted above, one of the chief criteria used by the Lake City police to determine whether to turn a juvenile over to the court is the extent of their prior contact with him. Consideration of the number and seriousness of prior and concurrent offenses markedly reduces the differential handling of juveniles of different races and ethnic groups. Table 6 indicates the extent of this reduction when simply considering the number of prior contacts with the court. Table 7 presents the dispositions when the seriousness of the current offense, the prior offense experience, and the concurrent offense experience are all held constant. The reduction of differentials in some categories means, of course, the concentration of the differential handling in other categories. Thus, Latin Americans and Negroes with extensive "records" are dealt with much more "extensively" (or harshly), on the average, than are Anglos with similar records.

d) The rate of delinquency in a neighborhood from which a juvenile

TABLE 6

Disposition of Offenses by Race, Ethnicity, and Prior Offenses Known to Court (%)

	Anglo	Latin American	Negro
Decision 1. Probation officer sends to court			
No prior offenses	10 (249)	14 (117)	16 (114)
One prior offense	28 (47)	28 (43)	27 (44)
Two or more prior offenses	23 (35)	33 (64)	62* (45)
Decision 2. Judge sends to youth authority:			
No prior offenses	21 (24)	50 (16)	22 (18)
One prior offense	23 (13)	83* (12)	83* (12)
Two or more prior offenses	37 (8)	81* (21)	96* (28)

Note.—Bases for percentages appear in parentheses

* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than 95

TABLE 7

Disposition of Offenses by Race, Ethnicity, and "Total Offense Experience" Associated with Each Offense (%)

	Anglo	Latin American	Negro
Decision 1 Probation officer sends to court:			
Total experience ranks 1 or 2	9 (115)	5 (61)	6 (47)
Total experience rank 3	11 (97)	12 (51)	9 (46)
Total experience rank 4, 5, or 6	19 (72)	30 (43)	27 (48)
Total experience ranks 7 or more	21 (47)	40* (67)	61* (62)
Decision 2. Judge sends to youth authority			
Total experience ranks 1 or 2	20 (10)	67 (3)	67 (3)
Total experience rank 3	36 (11)	17 (6)	50 (4)
Total experience ranks 4, 5, or 6	14 (14)	69* (13)	77* (13)
Total experience ranks 4, 5, or 6	30 (10)	85* (27)	71* (38)

Note.—Bases for percentages appear in parentheses

* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than 95

comes may be seen as an index of the extent to which the juvenile's delinquency seems likely to continue because it is supported by his peers or by a neighborhood culture which commonly produces delinquent behavior. Thus, coming from a neighborhood in which there is a high delinquency rate becomes a partial justification for court personnel to perceive that a child is in need of "treatment." Consideration of neighborhood rates of delinquency reduces the differential disposition by race and ethnicity most noticeably for those tracts where the rate is low. The data were also analyzed by volume of delinquency in each census tract. This analysis produced a more consistent reduction in differential disposition by race and ethnicity than did any other consideration taken alone. It may be that volume of delinquency in different parts of town affects the court officials'

TABLE 8

DISPOSITION OF OFFENSES BY RACE, ETHNICITY, AND DELINQUENCY RATE IN CENSUS TRACTS OF OFFENDERS' RESIDENCES (%)

	Anglo	Latin American	Negro
Decision 1: Probation officer sends to court:			
Eight lowest delinquency rate tracts	17 (72)	11 (9)	11 (9)
Eight middle delinquency rate tracts	16 (138)	25* (114)	30* (148)
Eight highest delinquency rate tracts	17 (42)	20 (93)	33 (40)
Decision 2: Judge sends to youth authority.			
Eight lowest delinquency rate tracts	27 (15)	0 (9)	0 (9)
Eight middle delinquency rate tracts	27 (22)	66* (29)	70* (44)
Eight highest delinquency rate tracts	0 (3)	84* (19)	77* (13)

NOTE - Bases for percentages appear in parentheses Number undefined on census tract = 77
* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than 95

handling of offenders more than does the more sophisticated analysis of rates of offenses.

e) It is appropriate, of course, for court personnel to take all these considerations into account when coming to decisions about disposition. Combining the considerations of the marital status of the offenders' parents, the total offense experience of the offenders, and the delinquency rates in the census tracts in which they live, and weighting these produces what might be called a "total considerations score." As we might anticipate from tabulations of the variables individually, the only statistically significant differences in the handling of juveniles of different race and ethnic groups by the probation officers are for those whose total considerations scores are quite high. The judge, however, seems to have varied his handling of individuals of different race or ethnicity at all levels of considerations scores. Table 9 bears directly on the hypothesis stated above, holding constant all the considerations permitted by the data. It suggests that the hypothesis must be modified and divided into two parts. (1) *Statistical control for the marital status of juveniles' parents, seriousness of offenses, the number and seriousness of prior and concurrent offenses, and rate of delinquency in the offenders' neighborhoods will reduce below statistical significance the apparent bias in the probation officers' handling of persons of minority-group status except when these considerations seem to call for extensive state action regardless of race or ethnicity.* This excep-

TABLE 9

DISPOSITION OF OFFENCES BY RACE AND ETHNICITY WHEN TOTAL OFFENSE EXPERIENCE, MARITAL STATUS OF PARENTS, AND DELINQUENCY RATE IN CENSUS TRACT OF RESIDENCE ARE TAKEN INTO ACCOUNT
(%)

	Anglo	Latin American	Negro
Decision 1 Probation officer sends to court			
Rankings 1–5	14 (102)	9 (46)	10 (50)
Rankings 6–8	19 (74)	13 (55)	17 (54)
Rankings 9–12	26 (35)	32 (44)	32 (28)
Rankings 13 or more	12 (25)	44* (43)	79* (38)
Decision 2. Judge sends to youth authority.			
Rankings 1–5	29 (14)	25 (4)	60 (5)
Rankings 6–8	21 (14)	43 (7)	67* (9)
Rankings 9–12	22 (9)	79* (14)	89* (9)
Rankings 13 or more	33 (3)	95* (19)	67 (30)

NOTE Bases for percentages appear in parentheses Number undefined on census tract alone = 60, number undefined on marital status of parents alone = 87, number undefined on census tract and marital status of parents = 17, total undefined = 164

* Probability that Negroes or Latin Americans are more likely to be sent to court or to youth authority greater than 95

tion is emphasized in table 9 by separating out that one-sixth of the offenses for which the considerations scores are the highest, that is, those with considerations scores of 13 or more. (2) *These same statistical controls have relatively little effect on the race/ethnicity distribution of decisions by the judge to commit offenders to the state youth authority.* This division of the hypothesis seems called for by the fact that four of the eight intergroup comparisons in table 9 actually indicate the Anglos were more likely than the minority-group members to be sent to the court for a formal hearing. All eight comparisons of the judge's actions indicate that minority-group members were more likely than Anglos to be sent to the training school.

4. Negroes are more likely to have formal hearings and/or be committed to the youth authority for their offenses than are Latin Americans. These differences are, themselves, statistically significant at several points, but the statistics are not presented in the tables. This pattern supports the general assumption that Mexican-Americans have a middle-status rank between Anglos and Negroes in communities in which both minority groups are present in sizable numbers.

DISCUSSION

The findings above are based on only one court in one city. This court had the same chief probation officer and only two judges during the time period

covered by the study. While these facts severely qualify the findings, they also mean that the artifacts of data collection are minimized.

Three questions arise readily from the findings.

First, do the data underestimate or overestimate the differential handling by persons of different race/ethnicity? It might be argued that the figures presented underestimate the actual race/ethnicity differentials in handling. The logic employed has been to control the data for some things (marital status of parents and place of residence) known to be associated with minority status as well as with high delinquency rates. Controlling in this way says, implicitly, that a decision which could have been made on the basis of, say, the delinquency rate in the neighborhood was made on that basis and not on the basis of the race or ethnicity of the offender. In the absence of contrary evidence, we have assumed that a high delinquency rate area or a broken home, for example, accounted for more extensive handling of a case.

On the other hand, it may be that the figures overrepresent the race/ethnicity differentials in handling on two grounds. (*a*) The considerations held constant may account for more of the differentials than the data demonstrate. For example, further subdivision of the offenses for persons with total "considerations" scores of 13 or higher might explain some of the differential (although it would involve such small numbers that any test of statistical differences would mean very little). The six offenses with total scores over 35, to illustrate, were all committed by minority-group members. (*b*) Appropriate considerations other than those held constant here may explain the differentials, especially attitudes displayed by the juveniles and/or their parents. It is appropriate, of course, that these attitudes should be taken as indicators of whether or not the juveniles are "hardened" in their criminal activity, and this consideration probably works against persons of minority-group position. Piliavin and Briar found that poor demeanor was perceived by the police to be characteristic of Negroes (see n. 2 above) but did not report whether the demeanor of Negroes was actually different.[12] It seems reasonable that minority-group members might actually be more hostile to the white-dominated correctional forces than would whites (for a brief discussion of minority resentment and numerous references, see Pettigrew 1964, pp. 35–47). Persons feeling such hostility would probably not be "contrite about their infractions, respectful to officers, and fearful of the sanctions that might be employed against them" (Piliavin and Briar 1964, p. 210). It seems reasonable that in the court as well as at the hands of the police minority-group members may display hostility which is interpreted by legal agencies as evidence

[12] The results of semantic differential tests (Short and Strodtbeck 1965, pp. 146–56) indicate that there really are such differences, at least in self-perception.

that the offenders and/or their parents are not anxious to conform to conventional standards of legality.

This perception may well have greater effect on the judge's than on the probation officer's decision. Probation officers have generally interviewed the juvenile and his parents more than once and often have interviewed the juvenile's teacher or employer before placing the case before the juvenile court. The judge's contact with the juvenile, generally confined to the probation officer's report and to the few minutes of the court hearing, might well be more influenced by the appearance and demeanor of the offender or his parents.

Second, assuming that the data as presented accurately reflect the amount of differential handling of persons of different race/ethnicity and assuming that no considerations other than those held constant are relevant, how much of the total differential may be accounted for by characteristics of the offenders and their offenses and how much by the race/ethnicity bias (discrimination) of those who handled the youths? A very rough estimate may be made by the following procedure. The four categories of total considerations scores were taken as ranks 1 through 4. A simplified analysis of variance of the data used to develop the figures for decision 1 (the decision to have a formal hearing) suggested that about 8 percent more offenses by Latin Americans and Negroes than by Anglos "should" have resulted in court hearings on the basis of their higher average total considerations scores. In fact, 6 percent more of the offenses by Latin Americans and 15 percent more of the offenses by Negroes than Anglos resulted in such hearings. This would suggest either that about 7 percent (11) of the Negroes' offenses resulted in court hearings because of racial bias against the offenders or that about 7 percent (16) of the offenses by Anglos did not result in court hearings because of racial bias in favor of the offenders. A similar procedure for the data about decision 2 (the decision to send individuals to the youth authority) indicates that about 15 percent more of the offenses by Latin Americans and by Negroes than by Anglos "should" have resulted in the offenders' being sent to the youth authority on the basis of their higher average total considerations scores. In fact, 50 percent more of the offenses by Latin Americans and 45 percent more of the offenses by Negroes than by Anglos resulted in decisions to send the offenders to the youth authority. This would suggest that either about 35 percent (15) of the offenses of Latin Americans and about 30 percent (17) of the offenses by Negroes which were subjects of formal hearings resulted in the offenders' being sent to the youth authority because of racial bias against them, or that about 50 percent (20) of the offenses by Anglos did not result in the offenders' being sent to the youth authority because of racial bias in their favor. It appears that total considerations scores as high as 13 (the

category in which most of those sent to the youth authority fell) would justify sending individuals for youth authority "treatment." The bias, then, appears to be one of not applying the law to the "privileged" race rather than one of applying it with excessive severity to the minority groups. This conclusion would, of course, be in accord with traditions extending back in time at least to the "right of clergy," which was applied eventually to all literate persons. Generalizing across the data on the two decisions, it appears that about two-thirds of the differential handling results from bias.

The third question that arises from the findings is whether or not the criteria on which considerations scores are based are really "appropriate." The appropriateness of these criteria is based on the relatively high probability of the continued delinquent behavior associated with these considerations. It is clear, however, that these considerations are, themselves, biased in favor of white middle-class persons. Broken homes and neighborhoods with high official delinquency rates are, of course, more characteristic of the lower than of high classes. Short and Nye's (1957, pp. 209–10) study referred to above, for example, indicates that coming from a broken home is more important in being judged a delinquent than it is in the commission of delinquent acts. It has been suggested that one latent function of our law-enforcement agencies is to enforce the norms of the middle-class "establishment" on lower social and racial ranks. That we recognize the considerations by which this enforcement is carried out as "appropriate" means that this function is actually manifest. Accepting these considerations as appropriate makes it virtually inevitable that those at the bottom of the power structure will be overrepresented in criminal statistics even though these considerations legitimize this overrepresentation.

The data presented here certainly do not refute the view that our correctional system works to the disadvantage of lower-rank and minority-group members (or to the advantage of the dominant rank and race). Even differences that are not large numerically at any one stage in the correctional process could add up to very important differences for the system as a whole. It does appear, however, that the juvenile court we studied makes only a relatively small contribution to these differences when considerations consonant with the philosophy of the juvenile court are taken into account.

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On the Interpretation of Class Consciousness[1]

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Marx's concept of class consciousness has frequently been employed in political sociology to state that the emergence of leftist radicalism in the lower classes is affected not only by feelings of frustration but also by whether individuals blame their condition on the social structure rather than themselves. Most contemporary formulations, however, fail to specify whether the hypothesis predicts purely correlational or interactive relationships between frustration, structural blame, and leftist radicalism. Data from two lower-class samples drawn at different times in Santiago, Chile, are used here to test the two versions. Results support the correlational, but not the interactive, interpretation. Implications and limitations of this finding are discussed.

Implicit in Marx's description of the political behavior of the proletariat are two related questions: (1) Why don't exploited groups rebel against their oppressors? and (2) What factors will cause them to rebel? Marx and Engels (1962) and Marx (1963, 1964, 1967) answered with the social psychological concept of class consciousness: recognition by the working class of the structural determinants of its situation and correct identification of the classes are major factors promoting the political struggle of the proletariat. Individuals who are "conscious" in this sense will be much more likely to participate in a revolutionary process than equally frustrated but non-class-conscious persons (Ollman 1968).

Marxist sociology has often pointed out that the emergence of a rebellious orientation among the exploited is strongly influenced by whether they hold the social structure responsible for their suffering. This proposition was already present in Weber's (1958, p. 184) statement:

> The degree to which *communal action* and possibly *societal action* emerge from the *mass actions* of the members of a class is . . . especially linked to the *transparency* of the connections between the causes and the consequences of the class situation. For however different life chances may

[1] The data on which this paper is partially based were collected under a grant from the Midwestern Universities Consortium for International Activities (MUCIA) The second set of data was collected by Eduardo Hamuy as part of a four-city study of stratification and mobility in Latin America. I wish to thank Professor Hamuy as well as the Social Science Research Institute of the University of Wisconsin for making these data available to me. I am indebted to Professors Donald J. Treiman and Edward L. Fink for their most valuable comments and criticisms of an earlier draft of this paper. Whatever errors the present version contains, however, are my sole responsibility.

> be, this fact in itself, according to all experience, by no means gives birth to *class action*. . . . The fact of being conditioned and the results of the class situation must be distinctly recognizable. [Italics in original]

In Dahrendorf's (1965) theory of class conflict, structural blame differentiates the class-in-itself from the class-for-itself; it is also employed in developmental theories such as Horowitz's (1967). Horowitz makes structural imputation of responsibility for one's frustrations the essential characteristic of "developing man"; it forms an important element of the "true believer" character as portrayed by Hoffer (1966) and is assigned a key role in the development of collective movements by such authors as Killian (1964), Lang and Lang (1961), and Smelser (1963). Finally, the notion has been useful in explaining and predicting leftist radicalism. For example, Leggett's (1964) research among Detroit Negroes, Morrison and Steeves's (1967) work on National Farm Organization (NFO) members, and Petras and Zeitlin's (1967) study of agrarian radicalism in Chile point to a significant influence of class consciousness or "structural discontent" on the emergence of radical attitudes and behavior.

The theory is usually used to explain political attitudes in working-class or lower-class settings. These formulations, however, fail to clarify the nature of the relationship among the variables involved. Two points are clear. First, there is one dependent variable—leftist radicalism—and two independent variables—frustration with one's life situation and holding the social order responsible for this situation. Second, the two independent variables are supposed to be positively correlated with the dependent one. But one crucial point remains obscure: is the theory limited to predicting significant relationships between each of the independent variables and leftist radicalism, or does it envision the combination of frustration and structural discontent producing a unique effect on the latter? In other words, does the notion of class consciousness lead us to expect a purely correlational or an interactive relationship between the independent variables and radicalism?

The correlational version is, of course, conceptually simpler, since it only asserts that the proportion of leftist radicals will be higher among the dissatisfied who blame the social order than among other groups. Thus, with reference to formerly unemployed Cuban workers, Zeitlin (1966) states:

> It is not only the contrast between their situation and that of employed workers [i.e., frustration] which makes them amenable to the appeals of radical politics but also the fact that they can so easily recognize the source of their problems to be in the concrete economic order [i.e., structural blame]. [P. 49]

And a few pages later:

> Recurrent unemployment and underemployment led to revolutionary

> politics among the Cuban workers in part because of their exposure to the possibilities of a better life during periods of regular employment and in part because the connection between their situation and the concrete economic order was so transparent [P 51]

Most attempts at an empirical test of the class-consciousness hypothesis assume, explicitly or implicitly, a correlational interpretation. Thus, tabular data showing higher proportions of dissatisfaction and structural blame among NFO members than among other midwestern farmers are presented by Morrison and Steeves (1967). Their remarks concerning these findings are appropriately limited to a correlational view:

> . . . movement participators differ from nonparticipators not only in their higher degree of dissatisfaction but also in the kind of dissatisfaction they exhibit, for instance, in beliefs about the nature and sources of difficulties in reaching aspirations . . . just as structural blame rather than self-blame characterizes NFO members, hope in an organizational structure designed to bring changes in the features of the larger system, rather than self-hope is their trademark. [P. 427]

When the three variables are cross-tabulated, there is a tendency to overinterpret higher proportions of leftist radicalism among highly dissatisfied, structure-blaming individuals as indicators of class-consciousness effects. Obviously, these tabular findings are essentially a restatement of the fact that the three variables are positively intercorrelated (Campbell and Clayton 1961). The hypotheses supported by such results, as well as the above statements, can be formulated as follows:[2] $r_{LF} > 0$, and $r_{LB} > 0$. By extension: $R_{L\ FB} > 0$, where $L =$ leftist radicalism, $F =$ frustration with personal situation, and $B =$ structural blame.

The interactive interpretation of class consciousness is of considerably greater theoretical importance. Above and beyond the additive effects on radicalism of personal dissatisfaction and structural blame, there is a unique "class-consciousness" effect in that the proportion of leftist radicals among dissatisfied structural blamers will be significantly higher than expected on the basis of the two variables alone, while the proportion of radicals among dissatisfied nonstructural blamers will be significantly lower than expected.

[2] The temptation is to label this version of the class-consciousness hypothesis "additive," a term naturally opposed to "interactive" This, however, would partially misinterpret the meaning of both the hypothesis and the notion of additivity Additivity stands for the prediction of equal intervals of effect on the dependent variable for each independent variable across levels of the other To my knowledge, no such interpretation of class consciousness has been formulated. Correlational versions of the theory do not explicitly rule out the possibility of interaction Rather than a logical alternative to the interactive version, they stand for a "looser" interpretation of relationships between the variables It does not seem a chance occurrence that, while evidence of bivariate association permeates the empirical literature, theoretical discussions support, for the most part, the existence of class consciousness as a unique, emergent phenomenon

In a different terminology, Marxist and neo-Marxist theoreticians have stressed this interpretation of class consciousness. The emergence of "consciousness," as a recognition of the objective causes of deprivation and suffering, is frequently perceived as a precondition for radical political activism. Without consciousness, working-class protest remains a narrow economic activity; it does not become a true political struggle. Thus, referring to the mass protests by Russian workers in the 1890s, Lenin (1929, p. 32) states:

> The systematic strikes represented the class struggle in embryo, but only in embryo They testified to the awakening antagonisms between workers and employers, but the workers were not and could not be conscious of the irreconcilable antagonism of their interests to the whole of the modern political and social system, i e it was not yet Social-Democratic consciousness In this sense, the strikes of the nineties, in spite of the enormous progress they represented, were a purely spontaneous movement.

Along similar lines, Dahrendorf's theory of class conflict (1965, p. 178) subordinates the emergence of classes as active political groups to the transformation of latent interests into manifest interests, that is, the realization by class members of their objective position in the structures of authority and reward-allocation of the society and of the correct identification of their collective enemy:

> Role interests are, from the point of view of the "player" of roles, latent interests As such they can become conscious goals which we shall correspondingly call manifest interest. . . They always constitute a formulation of the issues of structurally generated group conflicts In this sense, manifest interests are the programs of organized groups.

The "psychological formations" that Dahrendorf identifies with the Marxist class consciousness constitute the crucial factor determining the emergence, from class-in-itself, of "interest groups, the articulate programs of which defend or attack the legitimacy of existing authority structures" (1965, pp. 179, 184).

Radical political attitudes among the working classes are, therefore, the product, not of two isolated causal factors, but of the unique confluence of structural blame by a frustrated population. This interactive hypothesis can be represented by the inequalities $PL_{FB} > \hat{P}L_{FB}$ and $PL_{F\bar{B}} < \hat{P}L_{F\bar{B}}$, where:

PL_{FB} = observed proportion of leftist radicalism for frustrated structural blamers

$\hat{P}L_{F\bar{B}}$ = expected proportion of leftist radicalism for the same group on the basis of the added main effects of the independent variables

$PL_{F\bar{B}}$ = observed proportion of leftist radicalism for frustrated nonstructural blamers

$\hat{P}L_{F\bar{B}}$ = expected proportion of leftist radicalism for the same group on the basis of additive effects

The difference between these two interpretations of class consciousness deserves careful consideration. This paper attempts to clarify the issue by testing and comparing the two versions of this hypothesis in an appropriate setting. Data from two different samples drawn at different times from the population of Santiago, Chile, will be employed.

Test I

Leftist radicalism can be defined as an acceptance of revolution as a legitimate means to remove an order perceived to be unjust and to establish a regime which will improve the lot of the poor. The theoretical content of the class-consciousness hypothesis, as originally formulated by Marx and by most prominent exponents since then, requires that we concentrate on leftist radicalism, rather than on liberalism or generalized leftism, as the crucial dependent phenomenon. This, however, presents serious problems to researchers, since leftist radical orientations are often either illegal or nonexistent. Difficulties of measurement frequently lead to the dilemma of relatively valid indicators of an unwanted variable such as liberalism or general leftism versus poor measures of radicalism.

Chile, during the last years, has offered many advantages for the study of radical leftism. In contrast to those Western nations where extreme leftist movements are weak, Chile possesses a militant, extreme-left organization large enough to be considered the first or second political force in the country (Soares and Hamblin 1967; Zeitlin 1968; Portes 1970; Sanders 1970). This organization is identified as the FRAP (Frente Revolucionario de Accion Popular) and is formed by the alliance of the Communist and Socialist parties. In contrast to other countries—Venezuela, Guatemala, Colombia—where the radical left consists of guerrilla or other illegal groups, Chile has allowed its most radical groups to act within the institutionalized political framework. This has offered a unique opportunity to test hypotheses concerning leftist radicalism because of a numerically significant and militant extreme left and because of the possibility of ascertaining attitudes toward that movement through survey questions on political sympathy—indicators that in other places would yield, at most, measures of "general leftism."[3]

[3] It may be argued that the FRAP does not truly represent an extreme-left alternative, since, for many years, it has been committed to nonviolence, coexistence with bourgeois parties, and electoral politics. This was, in fact, the main accusation leveled

The data for the first test were collected in 1962 from a probability sample of 822 heads of families in Santiago. The survey was conducted by Eduardo Hamuy of the University of Chile as part of a four-city study of stratification and mobility in Latin America (International Data Library and Reference Service 1967). In keeping with the rationale of the hypothesis, which predicts the emergence of leftist radicalism as a consequence of class consciousness *in the working classes,* only respondents employed in blue-collar or minor-service occupations were considered. This reduced the size of the sample to 360.

Leftist radicalism (L) was operationalized as the arithmetic mean of three variables, each measured by a closed question:

1. The individual's report of his specific party preference. A score of 1 was given to preferences for the rightist Conservative, Liberal, and Radical parties. A score of 2 was given to preferences for the center-left Christian Democrat party. A score of 3 was assigned to FRAP preferences or to preferences for the Socialist or Communist parties.

2. The individual's statement about his general political preferences. A score of 1 was given to rightism, 2 to centrism, and 3 to leftism.

3. The individual's attitude toward the Cuban Revolution. Opinions of "bad" and "very bad" were scored 1. "Undecided" was scored 2. "Good" and "very good" were scored 3.

The zero-order correlation between party preferences and general political preferences was .48 ($p < .001$); between party preferences and attitudes toward the Cuban Revolution, .24 ($p < .001$); and between the latter and general political preferences, .34 ($p < .001$). Although not high, these correlations were judged substantial enough to justify combination into a single index.

Frustration with personal situation (F) was operationalized by a question asking how satisfied respondents were with their present occupations. Fixed alternatives ranged from "very dissatisfied" (assigned the highest score) to "satisfied." Structural versus nonstructural blame (B) was operationalized by a closed item asking whether respondents believed they had

against it by MIR (Movimiento Izquierda Revolucionaria), a militant, guerrilla-style organization formed by leftist intellectuals at the end of the 1960s. It is noteworthy, however, that neither the Communist nor the Socialist party ever renounced its declared intention of carrying out a revolution in Chile; that both parties—especially the Socialist—militantly supported and kept close contacts with the Cuban regime; and that socialist leaders had serious difficulties during the last years in keeping party members from abandoning conventional politics for guerrilla warfare. The FRAP commitment to elections was not a matter of principle but of pragmatic strategy, largely determined by its numerical strength and military weakness. For FRAP strategists, especially the highly sophisticated Communist leadership, electoral politics was merely the shortest route to the attainment of political power, a view amply confirmed by the 1970 presidential elections. (On the social bases of extreme-left parties in Chile, see Petras and Zeitlin 1967 and Zeitlin 1968, on the tactics and results of the election, see Sanders 1970.)

been given the necessary opportunities to succeed in life. Structural blame was identified with a "no" response and given a score of 1; nonstructural blame was equated with a "yes" answer and assigned a 0. Means, variances, and ranges for each variable are presented in table 1.

TABLE 1

MEANS, VARIANCES, AND RANGES OF VARIABLES EMPLOYED IN TESTS OF THE CLASS-CONSCIOUSNESS HYPOTHESIS

Variable	Mean	Variance	Minima	Maxima
Test I (1962):				
Leftist radicalism (3-item index)	1 717	0 627	1	3
Frustration	2 939	1.133	1	5
Structural blame	0 569	0 246	0	1
Test II (1968)				
Leftist radicalism (extreme-left party preferences)	0 385	0.487	0	1
Frustration (5-item index)	0 000	11 902	−8.150	6.439
Structural blame	0 539	0.249	0	1
Test III (1968) *				
Leftist radicalism (7-item index)	0 000	21 252	−5 926	10 716

* Indicators of frustration and structural blame in Test III are identical with those employed in Test II

The zero-order correlation between the two independent variables was .10 (N.S.). The correlation of frustration with leftist radicalism (r_{LF}) was .21 ($p < .001$) and that of structural blame with radicalism (r_{LB}) was .13 ($p < .02$). These coefficients are surprisingly small in view of the emphasis placed by theoretical formulations on these two factors as major determinants of radicalism. Nevertheless, correlations of the independent variables with leftist radicalism are statistically significant. In tabular form both reach significance beyond the .01 level. It may thus be concluded that, although associations are small, they are large enough to prevent rejection of the correlational version of the hypothesis.

It remains to be seen whether the interactive version makes a more important contribution to the explanation of the dependent variable. To test the interactive version of the hypothesis, it is necessary to establish a base line of additive effects against which to compare actual results. Only then can we examine whether, in fact, the pattern of results is due to an emergent factor transcending the simple sum of main effects. For this purpose, the expected proportions of leftist radicals for different levels of frustration and blame were computed on the basis of a dummy-variable regression equation (Suits 1957; Treiman 1966; Boyle 1970). Given the limited number of cases, frustration was dichotomized into "dissatisfied" or "very dissatisfied" versus all other categories. Further subdivisions

would yield cells with low or zero frequencies. Structural blame was employed in the form presented above. Leftist radicalism was dichotomized into those having a score of 3 (leftist) in all three index components (or at least two, provided the score in the remaining item was 2) versus all others.[4]

Thus, the corresponding regression equation takes the form:

$$\hat{L} = a + b_1F + b_2B, \tag{1}$$

where $\hat{L}$ is the expected proportion of leftist radicalism; F equals 1 if the person is frustrated with his situation and equals 0 otherwise, and B is 1 if he finds that the country has not given him the necessary opportunities to succeed in life and is 0 otherwise.

Finding the expected proportions of leftist radicalism on the basis of the additive effects of frustration and structural blame is a simple matter of adding to the intercept (a) the unstandardized regression coefficients (b's) corresponding to each combination. Thus, the expected proportion of leftist radicalism among those who are not dissatisfied but are structural blamers is $a + b_2$. Similarly, the expected proportion for those who are dissatisfied but do not blame the social order is $a + b_1$. Among those who are neither dissatisfied nor structure blaming, the expected proportion of radicalism equals a. Coefficients associated with equation (1) are $a =$ .1473, $b_1 =$.1023, and $b_2 =$ .0662. Expected and observed proportions of leftist radicalism and interaction effects (obtained by subtraction) are presented in table 2.

TABLE 2

EXPECTED AND OBSERVED PROPORTIONS OF LEFTIST RADICALISM BY LEVEL OF FRUSTRATION AND FOCUS OF BLAME—1962 DATA*

	UNFRUSTRATED		FRUSTRATED	
	Nonstructural Blame	Structural Blame	Nonstructural Blame	Structural Blame
1 Expected proportion	.147 (124)	214 (144)	250 (31)	316 (61)
2. Observed proportion	161	201	.194	344
Interaction (2) — (1)	014	— 013	— 056	028

* Figures in parentheses are raw-cell frequencies.

[4] Use of a dichotomous dependent variable may create difficulties, since it automatically violates some assumptions of tests of significance and since extreme proportions are hard to fit with linear regression For this reason parallel analyses of the data for Tests I and III were conducted by employing the continuous form of the leftist radicalism index Results, in both cases, were identical with those obtained with the variable dichotomized. Thus, it seemed preferable to employ the dichotomous version of the dependent variable, since results, in the form of proportions, are of more straightforward interpretation than index means.

The rationale of the class-consciousness hypothesis leads us to concentrate attention on the group of dissatisfied individuals. It is among them, the theoretical argument holds, that presence or absence of structural blame should produce significant interaction effects affecting political attitudes. As can be seen in table 2, there is in fact some interaction in these cells, and, more important, it is in the predicted direction: actual proportions of leftist radicalism are *lower* than expected among those who are dissatisfied but do not blame society for their situation ($PL_{F\bar{B}} < \hat{P}L_{F\bar{B}}$), and *higher* than expected among those who are dissatisfied and also blame society ($PL_{FB} > \hat{P}L_{FB}$).

The question is, however, whether these differences are large enough to justify nonrejection of the interaction hypothesis. An appropriate test would be to examine the increase in the multiple correlation coefficient, $R_{L\ FB}$, when an interaction term is added to equation (1). The resulting equation would be:

$$\hat{L} = a + b_1F + b_2B + b_3F.B, \tag{2}$$

where b_3 is nonzero only when both frustration and structural discontent are present. In the case of two dichotomous independent variables, only one interaction term is possible. Its addition has the effect of reproducing exactly the observed proportions in each cell. The value of b_3, the interaction coefficient, is .1106 (N.S.).[5]

If, in fact, the combination of frustration and structural blame forms the major determinant of leftist radicalism, it may be expected that the regression term associated with this effect will produce a multiple correlation significantly higher than that resulting from added main effects alone. From equation (1), $R_{L\ FB}$ is .1427 ($p < .03$); from equation (2), $R_{L\ FB(F\ B)}$ is .1535, indicating that a negligible increment is produced by the addition of an interaction term.

This result justifies rejection of the hypothesis. It can be concluded that the data lend no support to the notion of a class-consciousness interaction effect significantly affecting political attitudes.

Test II

The second test of the hypothesis is based on data I collected during 1968 in four lower-class settlements in Santiago de Chile. All of these areas were located in the marginal periphery of the city. Although not randomly

[5] Significance levels are omitted as tests of the interaction hypothesis because of the bias introduced by a dichotomous dependent variable. Nevertheless, it should be noted that none of the tests carried out with a continuous measure of leftist radicalism deviated markedly from those presented here. In both cases, interaction coefficients yielded, without exception, nonsignificant t-values.

selected, they were deemed to be typical of the major types of slums found in Santiago. A 10 percent simple random sample of family heads was drawn from each area. With less than 1 percent of refusals, the total sample size reached 382.

The dependent variable is operationalized in this case as the response to an item asking individuals which political party they trusted most. Answers were dichotomized into preferences for the FRAP, Communist, or Socialist parties (assigned a score of 1) versus preferences for other parties, or no preferences at all (given a score of 0).

Frustration with personal situation is operationalized as a summated index of standardized scores in five items, each providing four closed-response alternatives:

1. A question asking how present occupation compared with that held previously. Alternatives ranged from "much better now than before" to "worse now than before" or "unemployed at present."

2. A question asking how present occupation compared with that aspired to when *R* started working. Alternatives ranged from "better now" to "much worse now."

3. A question asking how present income compared with that aspired to when *R* started working. Categories varied from "earns more now" to "earns much less now."

4. A question asking how present situation compared in general with that aspired to when *R* started working. Alternatives were identical to those in item 2.

5. A question asking *R* for an evaluation of the financial situation of his family. Responses ranged from "earns enough; can save" to "does not earn enough; suffers great deprivations."

These items were selected after a two-step procedure involving selection by inspection of the most highly intercorrelated items among all possible indicators of frustration and a principal-components factor analysis in which, equating the first factor with the theoretical variable under consideration, those initially selected items loading higher on secondary factors than on the first factor or having low ($<.50$) loadings on the first factor were eliminated. Table 3 presents the intercorrelations between the final five items presented above.

Structural blame was measured by the same indicator employed in the 1962 survey: a dichotomous item asking respondents whether the country had given them the necessary opportunities to succeed in life. Again, structural blamers were coded 1 and nonstructural blamers 0.

Means, variances, and ranges for these variables are presented in table 1. The zero-order correlation between the two independent variables is .25 ($p < .001$). The correlation of frustration and leftist radicalism (r_{LF}) is .22 ($p < .001$); that of structural blame and leftist radicalism (r_{LB}) is

TABLE 3

ZERO-ORDER CORRELATIONS BETWEEN INDICATORS OF FRUSTRATION WITH LIFE SITUATION—1968 DATA

Item	X1	X2	X3	X4	X5
X1	...	.45	30	.31	.29
X2	.	...	30	.44	.31
X3	...	...	..	.35	25
X4	.	...	...	...	.47
X5	. .	...	...	...	..

NOTE.—X1 = comparison between present and past occupation, X2 = comparison between present occupation and initial occupation aspirations, X3 = comparison between present income and initial income aspirations, X4 = evaluation of present situation in general in comparison with initial aspirations, and X5 = evaluation of present income in relation to family needs

.26 ($p < .001$). Although not high, these coefficients lend somewhat stronger support to the correlational version of the class-consciousness hypothesis than those encountered before. Once again, statistically significant relationships in the predicted directions are detected between each of the independent variables and leftist radicalism.

The interaction hypothesis is also tested through the procedure outlined above. Again, it will be necessary, because of the limited size of the sample, to dichotomize the independent variables. The focus of the hypothesis centers on those who are frustrated with their situation in life. Thus, it is necessary to increase the likelihood that those classified as frustrated are really so. This rules out the inclination to use the mean of the frustration index as the cutting point. Instead, it was decided to classify as "frustrated" only those scoring one standard deviation or higher in the frustrated direction. This would increase confidence in the existence of a significant level of frustration in this group without limiting excessively the number of cases available for testing. Structural blame and leftist radicalism are, as seen above, originally dichotomous.

Coefficients associated with equation (1) in this case are $a = .2321$, $b_1 = .1753$, and $b_2 = .2296$. Table 4 presents the expected and observed proportions of leftist radicalism and the interaction effects on the basis of these additive results.

The pattern which emerges is essentially identical with that observed in the 1962 data. Again, there are noticeable, although not large, departures from additivity, and again they are in the predicted direction: $PL_{FB} > \hat{P}L_{FB}$ and $PL_{F\bar{B}} < \hat{P}L_{F\bar{B}}$.

To test for the significance of these differences, again an interaction term is added to the regression equation. The regression coefficient associated with this interaction (eq. [2]) is .2107 (N.S.).

The multiple correlation associated with the additive equation ($R_{L\ FB}$) reaches .2882 ($p < .001$). Addition of an interaction term ($R_{L\ FB(F\ B)}$) increases this figure to .2973, less than a 0.01 increment.

TABLE 4

EXPECTED AND OBSERVED PROPORTIONS OF LEFTIST RADICALISM (EXTREME-LEFT PARTY PREFERENCES) BY LEVEL OF FRUSTRATION AND FOCUS OF BLAME—1968 DATA*

		Unfrustrated		Frustrated	
		Nonstructural Blame	Structural Blame	Nonstructural Blame	Structural Blame
1	Expected proportion	232 (158)	462 (161)	407 (18)	637 (45)
2	Observed proportion	247	447	278	689
	Interaction (2) — (1)	.015	— 015	— 129	052

* Figures in parentheses are raw-cell frequencies

It may be concluded that, while departures from additivity in the direction predicted by the theory are detected, they are not sufficiently large to justify rejection of the null hypothesis. Thus, the prediction of an interactive class-consciousness effect affecting lower-class political extremism is, once again, rejected.

Test III

The same data employed in Test II afford a final test of the theory through use of a somewhat different operationalization of the dependent variable. This consists of a summated index of standardized scores in seven items tapping dimensions more general than the extreme-left party preferences employed above. These items were selected through a procedure identical with that employed in constructing the index of frustration and included the following:

1. Attitude toward the Cuban Revolution
2. Attitude toward breaking off diplomatic relations with the United States
3. Attitude toward establishing friendly relations with Cuba
4. Attitude toward expropriating the property of the rich
5. Attitude toward elections versus a popular revolt as the means of attaining social justice for the poor
6. Attitude toward preservation of past institutions versus a revolutionary sweeping away of the past as necessary for consolidating positive social change
7. Attitude toward the use of violence as a means of attaining social justice

Intercorrelations between these items, omitted here, were the highest

among all possible indicators of leftist radicalism in the data.[6] Means, variances, and ranges of the combined leftist radicalism index appear in table 1. Indicators of frustration and structural blame are identical with those employed in Test II.

Correlations of the index of leftist radicalism with frustration and structural blame are presented in table 5. Tables 5 and 6 summarize results of the three tests of the hypothesis. Both coefficients are again significant and in the predicted direction, thus lending further support to the correlational version.

TABLE 5

SUMMARY RESULTS OF TESTS OF CLASS-CONSCIOUSNESS HYPOTHESIS (CORRELATIONAL VERSION)*

	Zero-Order Coefficients		
	F	*B*	*L*
Test I (1962)			
F	...	10	.21 (001)
B	...	...	13 (.02)
L	...	...	...
Test II (1968):			
F	...	.25	.22 (.001)
B	...	...	26 (001)
L	...	...	...
Test III (1968):			
F	...	.25	.11 (.04)
B	...	...	.30 (001)
L	...	...	...

NOTE —*F* = frustration with life situation, *B* = focus of blame (structural vs nonstructural), and *L* = leftist radicalism

* Figures in parentheses indicate significance levels of coefficients relevant to testing the hypothesis. Levels of 10 or higher were considered nonsignificant.

Results of testing the interactive hypothesis are presented in table 7. The proportions of leftist radicals are defined as the proportions of respondents in each cell scoring one standard deviation or higher in the leftist radical direction in the general distribution of the variable.[7] Inter-

[6] In total, twenty possible indicators of leftist radicalism were examined with intercorrelations ranging from .01 to 59 Correlations among the seven selected items ranged from 19 to 54.

[7] This cutting point rather than the mean is selected for reasons identical with

TABLE 6

SUMMARY RESULTS OF TESTS OF CLASS-CONSCIOUSNESS HYPOTHESIS (INTERACTIVE VERSION)*

	Observed Minus Expected Proportions of Leftist Radicalism		Regression Coefficient Associated with Interaction Term	Multiple Correlations Associated with Additive and Interactive Equations		
Test	$PL_{FB} - \hat{P}L_{FB}$	$PL_{F\bar{B}} - \hat{P}L_{F\bar{B}}$	$b_{L(F B) FB}$	$R_{L FB}$ (1)	$R_{L FB(F B)}$ (2)	(2) − (1)
I (1962)	.028 $N = 61$	— 056 $N = 31$	1106 (N S)	1427 (03)	.1535	0108 (N S)
II (1968)	.052 $N = 45$	— 129 $N = 18$	.2107 (N S)	2882 (001)	.2975	.0093 (N S)
III (1968)	011 $N = 45$	— 029 $N = 18$	0465 (N.S)	2169 (001)	2179	001 (N.S.)

NOTE --F = frustration with life situation, B = focus of blame (structural vs nonstructural), L = leftist radicalism, and N = cell frequencies

* Figures in parentheses indicate significance levels of coefficients relevant to testing the hypothesis Levels of 10 or higher were considered nonsignificant

TABLE 7

EXPECTED AND OBSERVED PROPORTIONS OF LEFTIST RADICALISM (INDEX MEASURE) BY LEVEL OF FRUSTRATION AND FOCUS OF BLAME—1968 DATA*

	Unfrustrated		Frustrated	
	Nonstructural Blame	Structural Blame	Nonstructural Blame	Structural Blame
1 Expected proportion	.092 (158)	252 (161)	.140 (18)	.300 (45)
2. Observed proportion	095	248	.111	311
Interaction· (2) — (1)	.003	— 004	— 029	.011

* Figures in parentheses are raw frequencies.

action effects are once again in the predicted directions but smaller than those encountered before. The regression coefficient corresponding to this interaction (table 6) does not reach statistical significance nor does the increment produced by it in the resulting multiple correlation.

SUMMARY AND CONCLUSIONS

This paper reports an attempt to test two different versions of the hypothesis that the emergence of revolutionary orientations in the lower classes is affected not only by frustration and discontent but also by whether in-

those presented in relation to the index of frustration, that is, for ensuring the existence of leftist radical orientation among those so labeled.

dividuals place blame for their suffering on the existing social order or on structurally irrelevant factors.

One version of this hypothesis, implicit in most attempts at empirical testing, holds that the incidence of leftist radicalism will be significantly higher among those who are frustrated with their situations and who blame the existing social order than among other groups. This is a consequence of the combined effects of the two independent variables, frustration and structural blame, both of which have significant associations with leftist radicalism. This is the correlational interpretation.

The second version, most commonly associated with theoretical discussions of the hypothesis, holds that, beyond whatever main effects these independent variables have, they will have a significant interaction effect in that leftist radicalism will be inhibited among those who, being frustrated, do not blame society for their failures and will be stimulated among those who are frustrated *and* blame the social order for their situation. This is the interactive version.

Through use of two independent sets of data, three different tests of these hypotheses were performed. The results lent some consistent support to the correlational, but not to the interactive, version of the theory.

Should future research confirm present findings, it would certainly be advisable to substitute for predictions of a unique political effect—brought about only by the combination of two causal influences—the less ambitious statement that two variables (subjective feelings of frustration and imputation of responsibility for frustrations to the social order) are both positively related to leftist radicalism. And since both independent factors lead to radicalism, their combination will tend to produce a greater effect than each of them alone. The empirical support encountered by this version in this and previous studies, coupled with the absence of strong research support for the interaction hypothesis, appears to increase the likelihood that a modest interpretation of class consciousness will prove more accurate in the future.

However, the results presented here must be interpreted with some reservations. First of all, support of the correlational version of class consciousness is derived from coefficients which, although statistically significant, are rather small. Second, support of the theory is limited to the hypothesized existence of a *relationship* between each independent variable and leftist radicalism. Nothing has been shown in support of the assertion that frustration and structural blame are *causes* of leftist radicalism. It is, in fact, debatable whether the nature of the relationship among these three variables can be adequately represented by a simple recursive system.[8] Third, rejection of the interactive version of the theory

[8] Tests of the class-consciousness hypothesis do not exhaust the possible causal

is not categorical. Although not significant statistically, consistent departures from additivity in the direction indicated by the hypothesis were encountered. The possibility cannot be discarded that a class-consciousness interaction effect may actually be operating but is being obscured by the limitations and errors of survey measurement.

In relation to this possibility, two more general cautions seem in order. First, the data on which these conclusions are based are, if not temporally, certainly spatially and culturally specific. Second, my personal experience strongly indicates the need to treat conclusions based on political survey data with reservations, since measurement in this area is particularly susceptible to error. This is due, among other factors, to relatively strong interviewer biases and to respondents' uneasiness and reservations. The possibility in fact exists that the mechanisms of survey research may prove intrinsically incapable of grasping important aspects of the phenomenon of leftist extremism and may therefore lead to partial or unreliable measurement. This, of course, would invalidate ensuing conclusions.

For these reasons, findings presented above should be interpreted as tentative and requiring further examination. The entire paper may, in fact, be read as illustrative of the type of problem political sociology faces in the study of leftist extremism and related phenomena. The habitual final admonition concerning the need for more research acquires here a particularly urgent connotation in view of the large and long-unbridged gap between political theory in this area and empirical elucidation of its numerous, unresolved contradictions.

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relationships between frustration and structural blame, on the one hand, and leftist radicalism on the other Ensuing analyses will further explore these relationships according to alternative conceptualizations

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Marital Status and Political Alienation among Black Veterans[1]

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This study explores whether being married and having children reduces the political alienation among young black veterans. Political alienation was measured by a twenty-eight-item multidimensional Likert scale. Black interviewers were used to gather the data from seventy-nine single and 112 married black veterans in a southern metropolitan area. Age, socioeconomic status, length of time since discharge, number of weeks worked during the year, employment status, and status inconsistency could be expected to be highly associated with both marital status and political alienation. It was decided to examine and, if necessary, to control for these intervening variables. Only age was found to be significantly related to both marital status and political alienation. Therefore, covariance analysis was used to measure the independent effects of marital status on political alienation. The data confirmed the general hypothesis that single men would be more alienated than married men. When age was controlled, however, married veterans with children were found to be as politically alienated as single veterans. A tentative explanation is offered.

Segments of the young adult population in the United States have experienced an intense political socialization during the last decade. We can now say with some confidence that a large minority of blacks and upper-middle-class white youths have become activated, alienated, or radicalized. How enduring are these sentiments? Will they fade as these young adults get married and accept parental and occupational roles, or will young people remain politically alienated from established institutions and possibly engage in more extreme forms of political protest?

There is an old adage that "married men make poor revolutionaries." Sociologists have generally taken the position that the family is a conservative institution, and that marriage as a personal and social phenomenon restrains unorthodox sentiments and behavior (Parsons and Bales 1955). Before marriage, men, and to a lesser extent women, tend to have a fairly broad latitude of behavioral alternatives and responses; after marriage the behavior of individuals tends to be more circumscribed by the role expectations for husbands and wives.

[1] This research was supported, in part, by Florida State University Research Council grant 20-036. We would also like to express our gratitude to the Urban League of Jacksonville, Florida, and to the Institute of Social Research, Florida State University, for their cooperation in the study.

Despite this widely shared opinion, there does not seem to be adequate empirical research which tests the degree to which marriage stabilizes or reduces extreme sentiments and activities characterized by an active form of political alienation. Three different sources from the Kerner Commission Report indicate that rioters, when compared with the noninvolved, tend to be single (National Advisory Commission on Civil Disorders 1968). However, the differences are relatively small. For example, in the Newark study, rioters were more likely to be single (56.2 percent) than the noninvolved (49.6 percent). Furthermore, these studies do not indicate the extent of political alienation that would be conducive to participating in a wider range of protest activities. This investigation attempts to answer the question, "Does being married and a father reduce the levels of political alienation among a potential vanguard of black leaders—black veterans?"[2]

As a potential vanguard, black veterans represent a substratum of the young black male population who have clearly established their manhood and who are disproportionately drawn from the *organized* working class rather than the *unorganized* working class or black middle class.[3] Through the Selective Service system, the military is able to reject large numbers of the unorganized working class. For example, between 1950 and 1966 the black failure rates on the physical exam and the Armed Forces Qualification (AFQT) were 14.5 percent and 54.1 percent, respectively (Karpinos 1967). Many young middle-class and upper-class blacks have access to the elaborate system of college, occupational, and medical deferments available to the white middle class. Both the military experience and social class origins of black veterans suggest they may develop distinctive patterns of political alienation.

Because of its appeal and utility, the concept of alienation has been used to account for a wide range of human behavior. In its most general meaning, alienation refers to "an individual feeling a state of dissociation from self, from others and from the world at large; thus it may be defined as a loss of identity" (Gerson 1965, p. 144). Historically and in contemporary literature, alienation is frequently treated as a psychological property of the individual—a syndrome or trait with as much popularity as the "authoritarian personality" once enjoyed.

Within the present context, black political alienation has a more specialized meaning. We are not referring to the conceptualization that emphasizes estrangement from self and society. Our concern is rather with a manifestation of *active* political discontent that Olsen (1969) has distin-

[2] This study is part of a larger research project which focused on the readjustment of black veterans to civilian life (see Fendrich and Pearson 1970; Chiricos, Pearson, and Fendrich 1970)

[3] For the distinction between the organized and unorganized working classes, see Drake (1966).

guished from political apathy. Moreover, we are concerned with the beliefs and values of a collectivity united in opposition to the dominant society. In this form of political alienation, "the person feels that because of its very nature his social world is not worth participating in. Alienation is *voluntarily chosen* by the individual as an attitude toward the social system, as he realizes that the system does not provide worthwhile activities or goals in social life" (Olsen 1969, pp. 291–92).

We suspect that blacks would not only have specific objects of political distrust but also specific bonds of identity that make them a self-conscious collectivity. This type of collectivity has been described by Gamson (1968) as an alienated solidary group acting as potential partisans in the political process. By his definition, this group represents "collections of individuals who think in terms of the effect of political decisions on the aggregate and feel that they are in some way personally affected by what happens to the aggregate" (Gamson 1968, p. 35).

Because of the collective reinforcement of black political alienation, we would expect its manifestation to be different from that of white intellectuals, blue-collar workers, college students, or white veterans. Although there may be sizable alienated groups within all of these aggregates, the particular location of each group within the social structure suggests the ideological content, and the focus of discontent will be different. Skolnick (1969) identifies the major ideological components in contemporary black protest: self-defense and the rejection of nonviolence, cultural autonomy and the rejection of white values, and political autonomy and community control. While all the above groups may distrust one or more political authorities, this cluster of ideological components distinguishes black partisans from other alienated groups.[4]

As an aggregate, young black veterans are likely candidates for political alienation. Since the effective desegregation of the Armed Forces, the military has been considered the most "color-blind" institution in white society, offering an opportunity structure in which a man is evaluated and rewarded for his achievements alone. More recently, conflicting beliefs about the military have emerged. Black leaders have been critical of the

[4] Terms like black nationalism, militancy, and political alienation contain overlapping meanings. Our preference is for black political alienation It connotes a clearer rejection of the values and institutions of white America as well as being specifically antagonistic toward the larger political process The concept black nationalism suggests a withdrawal from the political process by either physical separation or abstaining from political participation. Some writers also suggest advocates of black nationalism are not willing to use a variety of confrontation tactics to modify the social structure (see, e g, Essien-Udom 1962; and Blake 1970). Militancy as a recently popularized concept tends to be used so loosely that it embraces too broad a range of activities Black leaders ranging from the most conservative to the most radical refer to themselves as militants Frequently, the call to militant action, however, involves little more than getting out the vote or engaging in other types of political reform

devastating distortion in national priorities produced by the military-industrial complex (Young 1967; Student Nonviolent Coordinating Committee 1966). Blacks have suffered disproportionate numbers of casualties and fatalities in Vietnam (McLean 1968). Many blacks began to wonder why they were fighting for democracy abroad when the real battle was at home. Martin Luther King (1967, p. 33) expressed the sentiments of many black leaders:

> We are taking the young black men who have been crippled by our society and sending them 8,000 miles away to guarantee the liberties in Southeast Asia which they have not found in Southwest Georgia and East Harlem So we have been repeatedly faced with the cruel irony of watching Negro and white boys on T.V. screens as they kill and die together for a nation that has been unable to seat them together in the same school. So we watch them in brutal solidarity burning the huts of a poor village, but we realize that they could never live on the same block in Detroit.

The solidarity between blacks and whites dissolves once off the battlefield. There have been repeated outbreaks of racial hostilities among the troops Both within the United States and abroad, black-power groups have been organized within the service.

At home the black community is experiencing increased ambiguities. In 1966, 35 percent of blacks opposed the Vietnam war because they felt they had less freedom to fight for; in 1969 the proportion was 56 percent. By a margin of two to one, blacks felt their sons and relatives were fighting a disproportionate share of the war. Seven out of eight believed Vietnam costs are hurting the "war on poverty" at home (Gallup 1969). It is little wonder the percentage of first-term reenlistments dropped (Llorens 1968).

Once the veteran returns to civilian life, he is exposed to a new set of conducive factors for becoming politically alienated. After serving in the military, these men develop a set of expectations for the rewards and recognitions due them as veterans. Yet the black veteran returns to a society that is racist by its own admission (National Advisory Commission on Civil Disorders 1968), and he is faced with the prospect of second-class citizenship. In a comparative study of black and white veterans, black veterans had proportionally fewer men enrolled in vocational and educational programs, lower weekly incomes, fewer skilled jobs, and higher unemployment (Department of Defense 1969). The absence of meaningful reforms and the strains of adjusting to second-class citizenship suggest the development of political alienation and potential participation in black radical movements.

Different black organizations and groups are opting for the use of constraints—in contrast to inducements or persuasion—as effective means to alter the social structure (Killian 1968; Oppenheimer 1969). Because of the ever-present danger of a harsh reaction from the larger white society, the

movement demands higher levels of political alienation and commitment. For young married veterans, it would seem the levels of political alienation would be reduced. First of all, involvement in a new marriage forces the focus of attention inward. Husbands have to be immediately concerned about being a good provider, spouse, and father (Liebow 1967). Second, radical sentiments that could lead to action have important consequences not only for the veteran but for his wife and children, for example, employer sanctions, imprisonment, or worse. Within his social interaction networks, it would be difficult for his associates to remain disinterested in the extent of his political alienation. Radical sentiments are likely to commit young men to patterns of social interaction and group activities that are possibly disruptive of patterns of family interaction and may even threaten the existence of the family as a social unit. Moreover, the young veteran's wife, relatives, and friends are likely to remind him of his family responsibilities and obligations. Because of these constraining forces, it was hypothesized that married men would express a lower degree of political alienation than single men. Moreover, since children create additional responsibilities, it was hypothesized that married men with children would express lower levels of political alienation than either single men or married men without children.

RESEARCH DESIGN

The data for this study were collected by personal interview from 191 black veterans during the fall of 1968 in Jacksonville, Florida. The lack of available sources of information which could provide an accurate list of all black veterans in the metropolitan area and limited funds demanded that a variety of sources be used to assemble a list of 772 black veterans, representing a hypothetical population of veterans.[5] Names and addresses of black veterans who had been discharged from active duty after January 1, 1963[6] were obtained from several local agencies having information on veterans by race, from an enumeration of every third household in eighty randomly selected city blocks having 75 percent or more nonwhite house-

[5] The major sampling problem of this study was the selection techniques for finding the rare element, the recently returned black veteran, in the larger black population. Since it was not economically feasible to construct a list of the total population elements, we had to sample from an incomplete special list (see Kish 1965) of a segment of the population created by obtaining the names and addresses of veterans from a number of sources. The worst problem of special lists is the *noncoverage* of elements missing from the list Instead of sampling from the population, a sample hypothetic population is used for sampling. The parameters of this population are the characteristics of those veterans that make up the special list.

[6] The cutoff point of 1963 was used to provide a time frame that could provide a range in adjusting to civilian life.

holds in 1960, and from three snowball techniques.[7] A one-third random sample was drawn, and 199, or 77.7 percent, were interviewed. Eight men who were interviewed were either widowed, divorced, or separated and, therefore, were excluded from the analysis. Of the fifty-seven persons sampled but not interviewed, twenty-eight had moved, twenty-five could not be located after at least two call-backs, two refused to be interviewed, and two interview schedules were invalidated.

We have one check available to determine how well our hypothetical population of veterans approximates the characteristics of black veterans in Jacksonville. Thirty interviewed veterans whose names were obtained from the eighty-block enumeration provided an estimate of the population parameters. These veterans were compared to the remainder of the sample on twelve variables: length of military service, year of military discharge, highest military rank, branch of service, age, number of weeks employed during the past year, marital status, employment, income, education, occupation, and a combined socioeconomic status index (SES).[8] There were no statistically significant differences between the two groups when compared on the first nine variables. However, those veterans obtained in the enumeration did have lower levels of educational attainment, occupation, and, consequently, lower SES. These results suggest the sample is slightly biased toward those veterans who have higher SES and may not be representative of the black veteran population. Therefore, the inferences that are drawn are tenuous because of the limitations of the sample. Within this context, however, it should be noted that only 14 percent of the sample were in white-collar occupations, 16 percent were skilled workers, 51 percent were either semiskilled or unskilled, 13 percent were unemployed, and 6 percent were full-time students.

The nature of the social structure of the black community, its suspicion of white motives, and its general mobility all present unique difficulties in sampling and interviewing black veterans. These were overcome in large measure by obtaining the cosponsorship of the Jacksonville Urban League and by using black veterans as contact men and field interviewers. Black veterans were trained to complete the enumeration, conduct twenty-five

[7] The snowball techniques consisted of: (1) sending stamped postcards requesting names and addresses of any black veteran who might qualify for this study to the first 400 names that were obtained, (2) requesting at the end of each interview the names of additional black veterans who were discharged since 1963, and (3) asking potential interviewers for additional names and addresses.

[8] The SES scores were computed for each black veteran from income, education, and occupational data obtained in the interview. The reported educational level and monthly income of a veteran were assigned nationally standardized scores developed by Nam and Powers (1965), using cumulative percentage distributions of income and education from the 1960 Census. Occupational scores were based upon 1960 Census data for income and education by occupation (Nam and Powers 1968).

pilot interviews under field conditions, and to collect the data for this study. Cooperation was obtained from a local television station and two radio stations to publicize and explain the general objectives of the study to the black community.[9]

Operationally, black political alienation is defined as an active rejection of white political figures and their institutions and values as well as the support of those beliefs and representatives of the black community that foster and encourage sentiments of black identification, militancy, and nationalism. Black political alienation is considered to be a multidimensional phenomenon, consisting of six interrelated dimensions. The first dimension is distrust in white political authorities. White political figures are regarded by many blacks as incompetent and inept in achieving the collective goals of black people and biased against blacks in handling conflicts of interest. The second dimension is the rejection of the public philosophy that is believed to pay lip service to the beliefs of justice, equality, and opportunity (Gamson 1968). The third dimension is positive black identification which refers to a set of attitudes that fosters a separate social psychological identity. This process is thought to supplant traditional white values, concerns, and definitions of self with alternatives that are principally or exclusively black. The fourth is support for leaders and organizations that have rejected traditional *reform* efforts and advocate, in their rhetoric and action, the restructuring of American society. The fifth is favorable sentiments for political and social separation of blacks from white America, and the sixth dimension is sympathetic support for the use of violence as a constraining influence on white society.[10]

Black political alienation was measured by a twenty-eight-item scale that consisted of six subscales.[11] Each item had five responses that allowed the

[9] Originally, it was felt that there would be hostility on the part of some veterans to interviewers We felt the mass-media coverage focusing on the Urban League's cooperation in the study and focusing on the readjustment problems would elicit greater cooperation The television exposure consisted of a video-taped interview in the Urban League Office with members of the research staff and Urban League officials A five-minute tape was shown on the "Evening News" the day before the interviewers went into the field. The Urban League had its own Saturday morning radio show in which the objectives of trying to ease the process of adjustment for black veterans was emphasized Spot announcements on a radio station directed toward the black audience were run while interviewers were in the field The announcements emphasized cooperation with interviewers who were trying to find out the adjustment problems black veterans were encountering There is no direct evidence of how effective was the media coverage A very low refusal rate (0 78 percent) indicates indirectly a high level of willingness to cooperate.

[10] These last four dimensions are quite similar to Skolnick's (1969) discussion of black militancy.

[11] Originally, there were twenty-nine items in the black alienation scale; however, one item in the dimension of black identification did not correlate strongly with the subtotal or total scores. It was decided to drop this item from the scale

respondent a degree of agreement or disagreement with the item, the extent of distrust in white authorities, or the degree of positive evaluation of black leaders and organizations. The estimated split-half reliability of the twenty-eight-item scale is .80. Item-subscale and item–total scale correlations indicate the scale is internally consistent. Interitem correlations within a subscale are higher than the average correlations with items on all other subscales. All subscale total scores are significantly related to the total twenty-eight-item total score. The mean and standard deviation of the scale are 87.50 and 14.82, respectively. The higher the score, the greater the black political alienation.

FINDINGS

Patterns of response to the items measuring black alienation may be noted in table 1. The dimension of distrust in white authorities apparently increases with closer proximity to the authority figures.[12] The two most distrusted authorities were policemen (57 percent) and labor leaders (37 percent). Only two white authorities were perceived with some degree of trust: congressmen (37 percent) and a member of a civil rights commission (56 percent). Rejection of current public philosophy increases as it becomes more abstract. Seventy-seven percent of the veterans believed that this nation is more concerned with law and order than with justice, and 67 percent thought the concept of equal opportunity was only a myth. Forty percent of the veterans believed they were being judged on their abilities instead of their race, which may indicate a perception related more closely to their individual experiences. The third dimension of black alienation measures sentiments favorable to political and social separation The general sentiments seem to be for blacks to be more divided on the local level than on the national level about their control of various social institutions. The trend is to prefer cooperation as they approach the national level of social involvement. For example, only 11 percent of the respondents believed blacks and whites can *never* live together peacefully Sympathetic support for violence as a constraining influence against whites drew substantial support. A high degree of apprehension seemed to be present as indicated by the 58 percent who agreed that a major race riot could easily break out in Jacksonville and the 36 percent who felt that violence is the only way for blacks to obtain their rights. The measure of positive black identification is our fifth dimension of alienation. Positive racial pride was illustrated by the 90 percent who believed African history should be part of the curriculum for black children, 65 percent who identified themselves first as black men and then as Americans, and the 50 percent who considered Afro hairstyles appropriate for black women.

[12] This same pattern has been noted by Clarke and Egan (1970) and Gamson (1968).

TABLE 1

RESPONSES TO THE ITEMS OF THE ALIENATION SCALE (IN PERCENTAGES)

Item	Trust (%)	Distrust* (%)
Distrust in white authorities:		
Policeman	14	57
Labor leader	24	37
Political party leader	22	31
Metro commissioner	18	26
Congressman	37	22
Member of Civil Rights Commission	56	13
	Agree	Disagree
Rejection of the public philosophy.		
Blacks today are being judged more on their abilities than on their race	40	48
People today are more concerned with law and order than with justice	77	12
In the United States the concept of equal opportunity is only a myth	67	20
Favorable sentiments for political and social separation		
If I owned a black business, I would prefer to hire a black man over a white man	40	38
Black people should run the schools in their neighborhoods	39	44
Blacks should work together as a separate group outside the two major political parties in order to gain more political power	38	49
Blacks and whites could never live peacefully together	11	73
Sympathetic support for violence as a constraint:		
It is becoming clear that violence is the only way for blacks to obtain their rights	36	47
Black-white relations are going to get much worse before they get better	33	46
Black leaders should only try to peacefully persuade white leaders to change their politics on race relations	52	30
A major riot could easily break out in this town in the near future	58	18
Positive black identification		
African history should be part of the curriculum for black children	90	5
I think of myself first as a black man and second as an American	65	31
Afro hairstyles are not appropriate for most black women	37	50
	Positive Evaluation	Negative Evaluation
Support for black radical leaders and organizations.		
Stokely Carmichael	43	44
H. Rap Brown	38	50
Malcolm X	37	45
Congress of Racial Equality	60	18
Student Nonviolent Coordinating Committee	58	23
Deacons for Defense and Justice	33	28
Black Muslims	33	52
Black Panthers	39	40

* Undecided responses were omitted.

The last dimension is the amount of support for radical black leaders and organizations. Three leaders and five organizations were selected because of their public rejection of the traditional reform efforts of the black moderates and their advocacy, in rhetoric and action, of the radical restructuring of American society. None of the leaders and only two of the organizations, the Congress of Racial Equality (60 percent) and the Student Nonviolent Coordinating Committee (SNCC) (58 percent), were evaluated positively by a majority of the veterans. (In the summer of 1969, SNCC dropped the word "Nonviolent" from its title and substituted "National.") While a majority of the veterans did not positively evaluate radical leaders and organizations, the lowest level of support was 33 percent for both the Deacons for Defense and Justice and the Black Muslims. It is also interesting to note the even split on the Black Panthers. Of those veterans who had opinions, 39 percent gave the Panthers a positive evaluation and 40 percent a negative evaluation. Of the three black leaders, Stokely Carmichael was given a slightly more positive evaluation (43 percent) than H. Rap Brown (38 percent) or Malcolm X (37 percent).

After examining the distribution of responses in table 1, it would seem that the majority of black veterans are not highly alienated. This pattern is similar to other studies (Marx 1967; Campbell and Schuman 1968; and Elder 1970) that have reported on protest sentiments. It should be noted, however, that the course of the black protest movement during the last fifteen years has never been determined by majority opinion. Instead, there have been vanguards within the population of young blacks that have been thrust into active forms of participation when their particular qualifications and skills became important to the evolving protest movement. Martin Luther King was only twenty-seven when he led the Montgomery bus boycott. Black college students were the leaders of the "sit-ins" in the early 1960s. Assuming a model of a two-step flow of communication, the large minority of highly alienated veterans could become the opinion leaders for the black community, particularly if black disenchantment with white America intensifies.

ANALYSIS

One of the major difficulties of measuring the independent effects of marital status on political alienation is the intervening effects of third variables that are significantly related to both marital status and political alienation. Within the limitations of the data of this study, six variables were considered possible intervening variables: age, length of time since discharge, SES, employment status, number of weeks worked during the past year, and objective status inconsistency.[13] Younger men are more likely to be

[13] Status-inconsistency research suggests that individuals with widely dissimilar statuses

single and also express stronger protest sentiments (Campbell and Schuman 1968). The most recently discharged veterans are also more likely to be single and have higher levels of political alienation because they served in the military during the development of black dissatisfaction with military service and the development of racial hostilities. As recent arrivals, they also experience more acutely the difficulties in readjusting to civilian life. Young men who have lower SES are more likely to be politically alienated (Street and Leggett 1961; Geschwender and Singer 1970), less likely to have delayed marriage, and, if they are married, more likely to have children. Young black veterans who have been unemployed for long periods during the past year and who are currently unemployed are more likely to be single (Department of Labor 1968) and have higher levels of political alienation. Similarly, young status-inconsistent veterans are more likely to be single and have higher levels of political alienation (Geschwender 1968). Therefore, before examining the relationship between marital status and black political alienation, it is necessary to examine the zero-order product-moment correlations with the possible intervening variables and marital status and political alienation.

Of the six possible intervening variables, only one, age, was found to be significantly related to both marital status and black political alienation.

TABLE 2

Zero-Order Correlations for Age, Marital Status, and Black Political Alienation

	Age	Marital Status	Black Political Alienation
Age	...	494*	— 286*
Marital status	..		— 187*
Black political alienation	...	...	...

* $p < 01$

Table 2 reports the associations between age and marital status (.494), age and black political alienation (—.286), and marital status and black political alienation (—.187). Three of the other variables—length of time since discharge, number of weeks worked during the last year, and employment status—were significantly related to marital status but not political alienation. Socioeconomic status and status inconsistency were neither significantly related to marital status nor black political alienation.

Since age has shown to be strongly associated with marital status, and

in education, occupation, and income may be highly alienated and prone to join social movements which attempt to change the basic structure of society (Geschwender 1968; Lenski 1954) when attempts to change oneself prove futile. Status inconsistency was measured by using the status crystallization technique first employed by Lenski (1954).

both are associated with alienation, it is impossible to determine the independent effects of marital status on alienation from the bivariate analysis among pairs of the three variables. The association between marital status and alienation may well be spurious, with age being the underlying cause of the association. In order to gain some insight into the problem, we turned to the analysis of covariance as a statistical procedure to assess the independent effects of marital status on political alienation by statistically standardizing the effects of age (Blalock 1960). The capabilities of the analysis of covariance as a model and statistical procedure in sociological research has been overlooked (Schuessler 1969), particularly within the context of elaboration, that is, studying the relationship between the distribution of X and Y in the light of a third variable Z.

In this study, we are able to relate an interval level measurement (alienation) and an ordinal measure (marital status), while controlling for the interval measure (age). The analysis of covariance can be used as an effective technique to assess the independent effects of marital status on alienation if there is not significant interaction effect; that is, age and marital status have an additive rather than a multiplicative effect on alienation. Therefore, in proceeding with the data analysis, we will report the level of interaction between the independent variables, the effect of each independent variable on alienation when the other is statistically controlled, the total amount of variance explained in black political alienation by age and by marital status, and the unadjusted and adjusted means of black alienation for single, married, and married with children.

Because of sampling fluctuations, some indication of interaction between the independent variables was expected. The test for interaction, however, was statistically nonsignificant (F-ratio $= 1.45$). We, therefore, concluded there was no significant interaction between the independent variables; that is, the relationship between an independent variable (marital status) and the dependent variable (black alienation) did not differ according to the value of the control variable (age).

The F-ratio in the analysis of covariance for each independent variable's relationship to alienation was statistically significant. Controlling for the effects of marital status, the F-ratio for the relationship between age and alienation was 11.43, which is significant at the .001 level of confidence. The total amount of explained variance in political alienation was 5.33 percent. Controlling for the effects of age, the F-ratio for the relationship between marital status and alienation was 5.00, which is significant at the .05 level of confidence. The amount of variance explained was 4.66 percent.[14] This pattern of relationships is similar to the zero-order correlations

[14] The question can be raised as to whether 4.66 percent of the variance explained is of substantive importance. It accounts for a relatively small amount of the total variation. Blalock (1968, p 157) notes, "that in the most sociological research we encounter

reported in table 2. In this analysis, however, we have an estimate of the *independent* effects of age and marital status on the dependent variable black political alienation.

Since we were interested primarily in the association between marital status and political alienation, table 3 reports the unadjusted and adjusted

TABLE 3

THE UNADJUSTED AND ADJUSTED MEANS OF ALIENATION BY MARITAL STATUS

		Alienation	
Marital Status	*N*	Unadjusted Means	Adjusted Means (Age Controlled)
1. Single	(79)	91.91	89 84
2 Married	(34)	81 12	80 85
3 Married with children	(78)	85 82	88 03

NOTE Tests of significance between adjusted means 1 vs. 2 vs 3, $F = 5\ 00$, $p < \ 05$, 1 vs 2, $F = 10\ 99$, $p < \ 01$, 1 vs 3, $F = 0\ 30$, not significant, 2 vs 3, $F = 6\ 59$, $p < .05$

means of alienation for single, married, and married veterans with children. The adjusted means reflect the statistically standardized effects of age. The unadjusted mean scores of alienation reveal that single veterans were more alienated than married veterans; however, veterans with children are more alienated than married veterans without children. This finding suggests that marital status may be curvilinearly related to political alienation [15] Moreover, controlling for age increases the level of alienation

substantial unexplained variation" There are three major reasons for this frequent occurrence: (1) the wrong set of independent variables have been selected for the analysis, (2) a very large number of variables operating independent of one another have significant effects on the dependent variable, and (3) measurement error may reduce the level of explained variation. In this study, we do not think we selected the wrong variables, but we do recognize that there are a wide range of independent variables that could effect the level of political alienation among black veterans (see Fendrich and Pearson 1970) We also recognize that the three categories of marital status (single, married without children, and married with children) represents a relatively crude level of measurement. Nevertheless, we do feel the effect of marital status on political alienation is of substantive importance Many young Americans are experiencing acute levels of political alienation in the process of political socialization It is important to know what happens to these individuals as they become older, more integrated citizens. To know that 4 66 percent of the variation in political alienation can be explained by marital status independent of age does suggest that marriage will at least temporarily modify extreme sentiments for many young people. Although the extent of variation explained is relatively small, it does compare favorably with the 1 25 percent of variance in occupational status that Blau and Duncan (1967) were able to explain by marital status.

[15] In table 2 a product-moment correlation coefficient of —.187 was reported for the relationship between marital status and black political alienation. It had been assumed that marital status was linearly related to both age and political alienation The above analysis of covariance suggests that would be a better measure of the association between marital status and political alienation. The η is 274, suggesting a stronger association than the original r which assumed linearity The η for marital status and alienation (.274) is about as strong an association as the r between age and alienation (—.286).

for married veterans with children. We were struck by the high level of alienation among married veterans who have children. Initially, it was expected that this group would be the least alienated because of the additional responsibilities and concerns associated with being a husband and parent. Instead, this group was more alienated than veterans who did not have children.

Because of the unexpected finding between marital status and political alienation, it was decided to examine separate analyses of covariance for the three combinations of pairs of marital status to determine if there were significant differences between the adjusted means on black political alienation. The F-ratio for the difference of adjusted means between single and married veterans without children was 10.99, which was significant beyond the .01 level of confidence. The total amount of explained variance in political alienation was 8.83 percent. The F-ratio for the difference of adjusted means between married veterans without children and married veterans with children was 6.59, which was significant beyond the .05 level of confidence. The total amount of explained variance was 5.33 percent. The third test of difference between the adjusted means of single and married veterans with children was not significant (.30). In sum, the single veterans and married veterans with children were significantly different from married veterans without children and quite similar to each other when age was controlled.

DISCUSSION AND SUMMARY

In this investigation, we attempted to explore whether being married and having children reduces black political alienation. It was hypothesized that there would be a decreasing level of alienation among married veterans and even more so among those veterans who were parents. Because age, length of time since discharge, SES, employment status, number of weeks worked during the past year, and status inconsistency were expected to be highly associated with both marital status and alienation, it was decided to complete a separate bivariate analysis of the associations among the variables before examining the independent effect of marital status on alienation. The zero-order correlations for the associations between age and marital status, age and alienation, and marital status and alienation were all statistically significant. The remaining five possible intervening variables were not significantly related to both marital status and political alienation. These findings suggested that covariance analysis controlling for age would be an appropriate measure of the independent effects of marital status on political alienation.

It was found that single veterans were more politically alienated than married veterans without children. The presence of children in the mar-

riage, however, was found to significantly alter the level of alienation among married veterans. Married veterans with children were found to be as alienated as single veterans when age was controlled. This finding contradicted part of our hypothesized relationship between marital status and alienation.

At present, we do not believe we have a definitive answer to why married black veterans with children should be more alienated than those married black veterans without children. Liebow (1967) observed unusual patterns of interaction between fathers and their children among the young unorganized working-class black males. He noted that family patterns began to deteriorate when inadequate jobs and income remained relatively constant but family responsibilities multiplied with an increasing number of children. Because of the inadequate economic opportunities to support a family, the manliness of these fathers was threatened; hence, a state of personal anxiety developed. One frequent reaction was to become hostile or indifferent toward their children when the fathers could not meet the family obligations.

Liebow's research was conducted in 1962 among *unorganized* working-class black males. It may be valid to assume that by 1968, particularly, for black veterans who have clearly established their manhood and who are members of the *organized* working class, they would be more likely to be critical of societal deficiencies rather than their own shortcomings as providers and role models to their children. They are not alone in holding these beliefs. Rainwater (1966), Billingsley (1968), and Farley (1970) have found the black family's deficiencies as a socioeconomic unit are due largely to extrafamilial causes.

With today's emphasis on involvement, particularly among the young black veterans, a cycle of political alienation may begin in late adolescence or early adulthood, be temporarily moderated by marriage, and become *revitalized* after assuming the responsibility of parenthood. When the young veteran is first married, he is probably more concerned with the adjustment he must make with his new bride, with her friends and relatives, than he is with larger social issues. This pattern of social retreat has been discussed by Slater (1963).[16] The recently married veteran with his bride may develop temporary patterns of work and play which somehow help him to temporarily escape the socioeconomic bondage that frequently engulfs young black couples. For these reasons, political alienation that develops when veterans are single could well become tempered.

When the children come, however, a veteran may develop a new perspective of himself and his family's future. He begins to experience more

[16] We cite Slater's work only to suggest the sociopsychological dimensions which may be involved in the establishment of a new marriage. We do not mean to imply a Freudian interpretation of the turning-inward process

severely the financial burden of being a father. He sees his children trapped in the same type of low-income ghetto environment that his generation experienced. If he looks into the distant future for his children, he sees little hope for the profound structural changes in society that are needed to provide his children better socioeconomic opportunities. He is convinced he lives in a racist society structured against him. Thus, because of his experiences in the larger society, the black veteran is less likely to blame himself for his children's fate than he is to blame society, and, in the process, he becomes again more alienated.

While this tentative explanation is plausible, a longitudinal rather than cross-sectional analysis is necessary to confirm the hypothesis of the cycle of alienation for young black males. Because of the cross-sectional nature of our study, only limited interpretations can be made of the findings. A longitudinal analysis would provide the opportunity to rule out alternative explanations. For example, the decisions to marry and not have children might be more common among the least alienated instead of the most alienated, suggesting that a process of self-selection is causing the differences in levels of black political alienation. We are also keenly aware of the need for comparable data on black nonveterans. Until that time, we are not sure whether the phenomena explored here are peculiar only to black veterans or are a variation of a theme found among young black males in a hostile white society. We look forward to future evidence to support or reject our tentative explanation.

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Observations on the Study of Crime Causation[1]

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Differential association theory, typological views, and other approaches to crime causation are noted. Situational and genetic-historical factors in lawbreaking are identified and contrasted. The major thesis of the essay calls for increased attention to situational pressures as they interact with other factors. A value-added orientation to crime causation is explored.

INTRODUCTION

A critical examination of criminology would show that relatively little has been learned about the causation of adult criminal behavior in the several decades since the death of Edwin H. Sutherland. On the whole, criminology *is at about the same place as far as knowledge of etiology is concerned* that it was in 1950. This situation is in marked contrast to the area of juvenile delinquency analysis, where a massive outpouring of theorizing and empirical research has taken place since 1955.[2] Indeed, some of the best work in the entire field of sociology has occurred in the area of juvenile delinquency. As a result, we know considerably more about causal processes in juvenile lawbreaking than we did two decades ago.

One line of development in criminology in recent years has been to eschew causal inquiry entirely, as reflected, for example, in the writings of Jeffery (1956). He avers that attention should focus on crime instead of criminals, so that, in his view, the most appropriate sociological questions are about the social processes through which groups in society manage to get some conduct norms converted into legal norms, as well as about the workings of the legal system and correctional organizations which enforce these norms and label persons as "criminals." Parallel views have been offered by Turk (1969). Also related to this development is the rise of a "sociology of criminal law," concerned with the development of laws, the workings of the legal system, and the like. The anthologies by Simon (1968), Chambliss (1969), and Quinney (1969) contain a good number of writings in this general vein.

[1] Revised version of a paper read at the annual meeting of the Pacific Sociological Association, April 1970 I am indebted to Peter Garabedian, Joseph F. Jones, and Dennis Brissett for helpful comments on an earlier draft of this paper. In addition, I would like to acknowledge the incisive comments of several anonymous readers who examined this article for the journal. I have endeavored to strengthen the paper in several important ways suggested by these comments.

[2] For a review of this material, see Gibbons (1970).

This discussion starts from the view that continued study of crime causation is desirable and is a proper topic for sociologists. While we agree that the "sociology of criminal law" and sociological analyses of correctional organizations ought to be encouraged, we believe that crime causation continues to be an important topic for sociological study.

Three main currents of work in the area of criminal etiology in the last twenty years can be identified. First, Sutherland's theory of differential association has continued to receive much attention. Second, specific and independent studies of certain offender patterns and other facets of criminality have been reported. Third, there has been a good deal of work on offender typologies, directing attention to the varieties of criminal behavior and collating a mass of data within a typological framework.

In the next section, these lines of work regarding etiology will be briefly examined, with an eye toward pointing out their limitations and shortcomings. Next, some new emphases which need to be stressed and some new directions which need to be taken if etiological progress is to be made are discussed. In particular, it is argued that some lessons may be learned from the delinquency literature, where motivational formulations of causation have been supplemented by arguments stressing situational elements in etiology.

CURRENT APPROACHES TO CRIME CAUSATION

Differential Association

Sutherland's theory of differential association holds that offenders are motivated to engage in lawbreaking and in that regard differ from nonoffenders. They are seen as having normal personality structures; they engage in deviance because of an excess of internalized conduct definitions favoring violation of law. These conduct norms were acquired through association with carriers of antisocial standards in social situations of differential social organization (Sutherland and Cressey 1970, pp. 71–112).

The differential association formulation was first put forth by Sutherland in 1939 and revised in 1947. In this lengthy period of time few investigations have been made to subject it to empirical verification, but these have produced indeterminate results. This ambiguity of findings is attributable to the lack of clarity and specificity of several key ingredients in the theory, including the terms "intensity" and "excess of definitions favorable to violation of law." Investigators have not been able to operationalize successfully these critical elements in the theory (see Short 1960).

More recently, attempts have been made to clean up the logical structure of the differential association argument (DeFleur and Quinney 1966), and to revise it in the light of modern psychological principles of learning (Burgess and Akers 1966). Although it is too early to evaluate these

efforts in behalf of differential association, to date it does not appear that much progress has been made. I predict that insofar as this formulation is rendered testable by these revisions, the ultimate effect will be to demonstrate its inadequacy as a causal argument. Some new ways of looking at criminality are called for, rather than revisions in differential association theory.

Specific Criminological Studies

Certainly researchers have not been totally inactive in the area of crime causation. Instead, various sociology journals along with publications such as the *Journal of Criminal Law, Criminology and Police Science, Crime and Delinquency*, and the *Journal of Research in Crime and Delinquency* have contained many contributions directly or tangentially concerned with criminal etiology.[3] There has been a modest, continued growth apparent in the criminological literature. For example, Einstadter (1969) has investigated a number of armed robbers. He reports that robbers—unlike professional thieves—are not organized into a behavior system. Their robbery skills are not acquired through differential association but are instead parts of conventional culture which are assimilated Robbers infrequently exhibit backgrounds of involvement in gang delinquency. Many recruits to armed robbery are from the ranks of "night people" and underworld figures such as "pimps" and the like.

One problem with many of the specific studies in criminology in recent decades is that they have been relatively ad hoc in character, unconnected to one another by linkages to a common theoretical framework. Each study has provided useful descriptive data, but they have not tended to confirm some general theory of causation, nor has any broad etiological formulation grown out of them. These studies have rarely broken new ground or opened up new leads in the manner of Sutherland's investigations of "white collar crime" (1949). While these inquiries have been useful and helpful, they have tended to confirm traditional generalizations to some extent, instead of serving as stimuli to new approaches and discoveries in crime causation. The combined impact of these works has been to alter the structure of criminological thought a modest amount.

Typological Arguments

One prominent contention in criminology in recent years has centered on typologies. It has been maintained that real progress will come about when the markedly diverse population of offenders is separated into more homogeneous patterns or types. Each of these lawbreaker patterns would

[3] A generous sample of this work is contained in Gibbons (1968, 1970).

then be studied separately, so that etiological factors would be identified for the various types. Presumably, the causal ingredients operating in one form of criminality would be different from those in other patterns. The end product of this kind of work would be a set of miniature criminological theories, each dealing with particular patterns of criminal behavior, rather than some overarching formulation, such as differential association, which endeavors to account for "criminal behavior" as a class of phenomena.

One example of this orientation is the work of Clinard and Quinney (1967), where a large body of evidence was drawn together under a series of rubrics designating types of crime, such as violent personal crime, occupational crime, public order crime, and organized crime. Ferdinand (1966) has pulled together a large number of works on delinquent offenders in the form of an extended essay on delinquent types. Also, Roebuck and Cadwallader (1961) purport to have uncovered a number of offender types made up of individuals exhibiting similar constellations of lawbreaking conduct. Finally, this orientation looms large in several of my books, where offender typologies designed to classify a large number of real criminals and delinquents into homogeneous types are advanced (Gibbons 1965, 1968, 1970).

Surely lawbreaking is comprised of a vast and heterogeneous collection of deviant acts, as all would agree. At the same time, common-sense observations seem to point to homogeneous types within the lawbreaker population. Moreover, it is possible to find cases of specific criminals who appear to be "professional thieves," "naive check forgers," or other types which have been identified in the criminological literature (see Einstadter 1969), even though many typological models are relatively terse and vague as to the identifying marks. The heuristic value of typologies is clear enough; they direct attention to more specific hypotheses about causation than differential association and the like.

But some words of caution are in order. There is the danger that the typological approach may be pushed beyond heuristic usefulness. These classification schemes might lead to a criminological picture of offender behavior which oversimplifies and distorts the nature of criminality, and perceives types as more crystallized and distinct than they are. This might come about as criminologists go about picking and choosing behavioral observations which fit a typological perspective while ignoring discordant data.[4]

[4] That the real world is populated by diverse kinds of lawbreakers might be suggested from an examination of Mayhew's classificatory efforts in nineteenth century England He produced a categorization involving 100 different types. Within his general category of "Those Who Plunder by Manual Dexterity, by Stealth, or by Breach of Trust," Mayhew enumerated "Stock Buzzers," "Tail Buzzers," "Prop Nailers," "Thimble Screwers," "Drag Sneaks," "Snoozers," "Sawney Hunters," as well as a number of other types (Tobias 1967, pp 62–64). Sawney Hunters were those

A recent study (Hartjen and Gibbons 1969), is germane to this point In it, a fairly explicit and detailed criminal typology was subjected to empirical examination in a probation setting. We used quite crude procedures, and only about half of the probationers were found to fall within a particular typology category. If more refined research techniques had been utilized, even more difficulty might have been experienced in trying to assign actual offenders to pigeonholes in the typology. The half of the probation sample that did not initially fall into the typology was subjected to further study. These individuals were sorted into some rough types such as nonsupport cases, property offenders, and so on. However, these "types" were not clear cut; these offenders were not much different from each other in terms of social backgrounds, nor did they appear to be very different from most law-abiding citizens. Hartjen and I pointed out that much of this lawbreaking appeared to be of the "folk crime" variety (Ross 1961), in which offenders were not involved in a criminal career. For example, one-quarter were nonsupport cases who had failed to comply with a civil court order to provide child support. Such offenders do not get much attention in contemporary versions of typologies, nor are they the subject of most causal arguments currently fashionable in criminology. But if they exist in the real world, their existence will ultimately have to be reflected in criminological thinking.

Moore and I (1970) found similar findings in an investigation of federal probationers. Several hundred probationers were sorted into "types," such as mixed property offenders, selective service violators, income-tax offenders, and car thieves. But these types also seemed not much different from each other, nor did they seem to be markedly unusual or antisocial kinds of citizens. Most of them had been involved in transient criminal episodes, but were reestablished as law-abiding citizens while on probation.

The point is that the existence of a broad class of offenders who might be labeled "situational-casual" criminals ought to be entertained.[5] These

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who go about purloining bacon from cheese-mongers' door steps! Clearly, Mayhew encountered some difficulty in generating a parsimonious typology, so that one might wonder what kinds of obstacles twentieth-century efforts in this direction might find.

[5] Actually, "situational" offenders have been acknowledged in a number of criminological essays. For example, Corsini (1949, pp. 110–11) has contended that "situational criminals" constitute one type among a number of patterns observed in penal settings. At the same time, he argued that only those persons who have an inadequate comprehension of the situation are driven to criminality, so that adverse circumstances alone are not sufficient to impel an individual toward lawbreaking. Along the same line, Barnes and Teeters (1959, pp. 53–54) have listed the situational criminal as an offender type, but their commentary is quite brief and superficial. In general, those who have spoken of situational offenders have usually dealt with this category as a residual one within which few persons would be placed Additionally, little

persons are involved in various forms of short-run criminality without falling into any clear-cut role-career. In turn, they may be the products of situational forms of causation. The judicious application of typological formulations is called for, by applying them only to those individuals who seem to be involved in career criminality, rather than trying to force all of the facts of lawbreaking into this typological mold.[6]

Concluding Remarks

Differential association theory was described above as providing an answer to the query, how does someone become the kind of person who commits a crime or the kind who refrains from lawbreaking? The differential association perspective asserts that offenders learn definitions favoring violation of law from their social association. Noncriminals acquire law-abiding sentiments from a different set of experiences. This question and the answer to it is actor- or person-oriented, so that it is assumed that various learning experiences operate to put some relatively specific kind of criminal motivation or tendencies inside a person. According to this framework, deviation does not occur without motivation to deviate I have suggested that if differential association theory could be tested, it would probably turn out to be inaccurate for many lawbreakers. Differential association may be an answer to a defective question that assumes that motivation to criminality must be inside the individuals who engage in criminal conduct. Then too, in differential association theory, definitions favorable to criminality are seen as absent or present in attenuated form in law-abiding individuals, so that they hold an excess of definitions unfavorable to violation of law (Sutherland and Cressey 1970, pp. 75–76) However, my contention is that many offenders may be no more motivated

systematic analysis of various situational pressures and inducements to criminality has been pursued

[6] Another point which ought to be made about typologies is that two kinds of role careers or offender types may exist It may be that some "types" are individuals who get caught up in continuing involvement in deviance out of some kind of genetic or historical process such as differential association (e g, professional thieves, certain kinds of sex offenders, and some other lawbreakers). There may also be persons who become engaged in a long-term career in deviance, in part as a *result* of correctional handling They may have been unable to extricate themselves from lawbreaking, once having acquired the stigmatizing identity as a "criminal" There is much speculative commentary holding that correctional institutions are "crime schools," that they have reformative effects upon some persons, or that they are essentially benign in impact upon criminal persons Similarly, speculative claims abound that other correctional experiences have certain effects. But if situational elements loom larger in etiology than has been assumed in the past, there is a critical need for expanded research attention to correctional experiences as career contingencies or situational influences. Speculation must be replaced by hard facts Some effort to organize the existing evidence bearing on this point can be found in Gibbons (1968, pp. 236–40; 1970, pp 221–60).

to engage in criminality than nonoffenders. Their lawbreaking behavior may arise out of some combination of situational pressures and circumstances, along with opportunities for criminality, which are totally outside the actor.

Typological formulations concerning criminals have a closely related deficiency. Typological thinking implies that instead of a single learning experience through which criminal conduct definitions are acquired, different, distinguishable learning experiences produce offender types characterized by separate motivational patterns. My argument is that many lawbreakers do not fit into any role-career kind of typology, that they lack clear-cut criminal motivation and do not derive out of a distinct etiological background. Many of them engage in "folk crimes" or other situationally induced forms of deviance.

If this argument has merit, then criminological attention ought to shift somewhat away from person-oriented perspectives toward more concern with criminogenic situations. It may be that many offenders are virtually indistinguishable from other citizens at the point of initial involvement in deviance, so that traditional views of causal relationships may not hold for many contemporary offenders.

In brief, the argument is for a reexamination of Sutherland's distinction between genetic and situational forms of causation. First, however, let us see what can be gained from recent contributions to the delinquency literature, where motivational arguments have been challenged by situational ones. The review of delinquency work will necessarily be terse and incomplete.

NEW DIRECTIONS IN CAUSAL THEORY

Recent Delinquency Theory and Research

The period since 1955 has been one of prominent growth in delinquency theory and research, particularly in regard to working-class, gang, subcultural delinquency.[7] Cohen's *Delinquent Boys* (1955) was the seminal statement which touched off this surge of work. He argued that gang delinquents were motivated by strongly held feelings of *status* discontent, so that offenders were thought to have shared perceptions that others think ill of them. Working-class youths were regarded as particularly susceptible to these anxieties about social status.

Cohen's essay was followed by a good many others, but the most important alternative formulation was that of Cloward and Ohlin (1960), holding that gang delinquents are motivated by *position* discontent rather

[7] Much of this theory and research on gang delinquency is summarized in Gibbons (1970, pp 102–41).

than status worries. In their view, working-class boys have high aspirations for material success, while they have only modest expectations for achievement. Those unwilling or unable to reduce their aspirations are the most likely recruits to the delinquent subculture.

These formulations eventually were attacked by those who argued that it may not be necessary to posit strongly held motivation to deviance in order to explain delinquency.[8] For example, Matza (1964) offered a portrait of delinquents as "drifters" into misconduct whose usual attachment to social norms is temporarily broken, and whose neutralizing rationalizations allow them to exculpate themselves for behavior they condemn in principle. Briar and Piliavin (1965) made similar claims, contending that motivational formulations overexplain, that they do not account for real delinquents who fail to exhibit the postulated motivation, nor for spontaneous reform or disengagement from delinquency. Briar and Piliavin proposed a picture of delinquency causation stressing lack of commitment to conformity, rather than positive motivation to deviance. The relatively attenuated stake in conformity shown by delinquents was traced to certain social class factors and class-related family experiences.

The findings of much of the delinquency research of recent years has supported these latter views more than the motivational arguments On the whole, the contentions of Cohen, Cloward and Ohlin, and other motivational theorists have received little empirical support (see Gibbons 1970, pp. 102–41). Instead, a picture of delinquents emerges which stresses their lack of social skills, lack of commitment to conformity, and provocative life circumstances which are conducive to juvenile misbehavior.

One major body of research evidence is that of Short and Strodtbeck (1965), derived from a series of investigations in working-class areas in Chicago. The delinquents they studied generally endorsed middle-class values and standards of "proper" behavior, although they were somewhat more tolerant of "bad" behavior than were nongang boys (pp. 47–76). These investigators also report that delinquents showed limited social skills of various kinds, intelligence scores that put them below nonoffenders from similar backgrounds, and anxieties in the area of sexual adequacy (pp. 217–47). Presumably, these youths had fewer personal resources with which to confront their life situation and a reduced commitment to conformity as a consequence. Short and Strodtbeck interpreted much of the aggression and other misbehavior exhibited by these offenders as a response to threats to their fragile sense of status and well-being gained from membership in gangs. Thus, much of their misconduct was designed to preserve their reputations as courageous fighters or members in good

[8] An excellent review and analysis of "strain" and "cultural transmission" varieties of motivational theory, along with "social control" arguments, is contained in Hirschi (1969, pp. 3–34).

standing in a gang, rather than serving as an attack upon the societal order perceived as unjust. Finally, some of the actions of delinquents—such as becoming fathers of illegitimate children—were interpreted as "aleatory" or chance phenomena, arising accidentally out of the frequent sexual involvement of the youths, rather than from deeply felt motivational sources (Short and Strodtbeck 1965, pp. 27–46).

Several other lines of evidence support the interpretation which links delinquency to lack of commitment, negative life circumstances, and the like, rather than to positive motivation toward deviance. Downes's (1966, p. 257) study in London of working-class delinquents concluded: "Their illegal behavior seemed to be due not to 'alienation' or 'status frustration,' but to a process of dissociation from middle class dominated contexts of school, work and recreation." Finally, Hirshi's (1969) recent investigation in Richmond, California, turned up a good deal of support for the social-control portrait of the delinquent as lacking in commitment to conformity, and with attenuated attachments to parents, school, and peers, so that he is psychologically more "available" to engage in sporadic acts of deviance.

The major implication of this work for the study of crime causation should be clear: perhaps more attention ought to be given to the possibility that a number of kinds of criminality are the work of individuals who are not characterized by any sort of clear-cut motivation toward lawbreaking, that their behavior is not the outcome of some kind of differential learning. Instead, there may be a number of forms of criminality in which situational elements loom much larger than acknowledged heretofore. Perhaps it is time to reexamine Sutherland's distinctions between situational and genetic explanations, giving more stress to the former than did Sutherland himself.

Situational versus Genetic Causation

Ironically, it was Sutherland who offered a major alternative perspective on etiology, as well as his ideas about differential association. He noted that differential association was a specific case of the *historical* or *genetic* views of causation, which he contrasted to *mechanistic* or *situational* perspectives (Sutherland and Cressey 1970, pp. 74–75). The genetic approach seeks those factors in the earlier life history of the criminal or delinquent to which his lawbreaking can be linked. The mechanistic-situational-dynamic view, on the other hand, examines processes operating at or near the moment of the crime. In general, a situational perspective assumes that the causal process operating in some instances of criminality is one which grew out of events closely tied to location and time of the deviant act.

The genetic approach has dominated criminological inquiry. Countless studies have attempted to ferret out the earlier life experiences which propelled persons down criminal paths By contrast, examples of causal investigations of a situational orientation are less common; Cressey's (1953) examination of rationalizations prior to the deviant act of financial trust violation is one such case.

While Sutherland identified situational or mechanistic causation, he gave it scant attention. He declared: "The objective situation is important to criminality largely to the extent that it provides an opportunity for a criminal act" (Sutherland and Cressey 1970, p. 74) Different persons will define the same objective situation differently. For some, it is viewed as conducive to criminality; for others, it is not. This is another way of arguing that only those motivated to engage in lawbreaking will do so when confronted by particular situations. In Sutherland's view, these definitions of the situation arise out of prior life experiences of the person, so that a historical or genetic explanation is required to account for the ways in which criminals define their current situations. If this is so, the fundamental etiological task remains to account for the long-term development of the person, his attitudes, and his motivational patterns. In all of this, the image of the offender is that of a person who is different, at least in social-psychological terms, from the nonoffender.

The thesis in this paper is that Sutherland overstated the matter by assuming that the earlier life experiences of offenders, extending over a lengthy time period, are always implicated in criminality. Contrary to Sutherland's assumption, a long-term genetic process may not always be found operating conjointly with situational elements to create lawbreaking. Instead, in many cases, criminality may be a response to nothing more temporal than the provocations and attractions bound up in the immediate circumstances. It may be that, in some kinds of lawbreaking, understanding of that behavior may require detailed attention to the concatenation of events immediately preceding it. Little or nothing may be added to this understanding from a close scrutiny of the early development of the person.

The argument here is *not* for an either-or choice between genetic or situational processes in causation. Instead, the problem for criminologists is one of explicating those relationships where genetic factors weigh most heavily, those where situational elements are most crucial, and those where both sets of influences seem to conjoin to produce the offensive behavior. The end product of this kind of work is likely to be an etiological formulation of considerably greater complexity than those now extant. A number of forms of genetic and situational causation, along with various combinations of these processes may ultimately be identified.

Genetic Processes in Criminality

Some instances of criminality can be observed in which historical or genetic factors seem to be quite powerful and in which situational elements are of minor significance, such as certain types of deviant sexual conduct involving exhibitionism, voyeurism, or sexually aggressive acts of a pronounced character (Gibbons 1968, pp. 367–405). Concerning these forms of conduct, Gagnon and Simon (1967, p. 9) have asserted: "In these cases, the causal nexus of the behavior appears to exist in the family and personality structure of the individual and is linked to the contingencies of his biography rather than those of social structure." Aggressive law-breaking of the kind often labeled "psychopathic" appears to be another form of criminality which arises out of genetic factors—in this case, severe and early parental rejection (Gibbons 1968, pp. 361–65).

In Sutherland's theorizing, genetic processes in causation revolve around some kind of associational history in which offenders have acquired definitions favoring law violation from their peers or other persons with whom they interact. Some criminologists (e.g., Glaser 1956) have modified the argument by claiming that some criminals learn definitions from persons with whom they identify but with whom they may not be in direct contact. But there is still another form which genetic etiology may take, in which stable careers in law violation emerge from initial flirtations with deviance, in which criminal definitions emerge in the process, and in which the offender may supply his own reinforcement of these emerging sentiments.

Naïve check forgers come to mind as one illustration of this possibility. It may be that the check writer's initial discovery that he can "kite" a check and subsequently settle the matter informally with the store that he has victimized may lead him to conceptualize check writing as a harmless problem-solving technique which is at his disposal. Interaction with others who supply such rationalizations as "You can't kill anyone with a fountain pen" is not involved in the unfolding of this career line in deviance.[9]

[9] Several other examples of this kind can be offered. Generalized attitudes holding that stealing is "bad" may become attenuated in amateur shoplifters who start off by experimenting with stealing To the extent that they manage to avoid detection, they may come to "normalize" shoplifting in their own minds Another illustration might be taken from Humphreys' study (1970) of impersonal homosexual acts in "tearooms." His data suggest that involvement in this behavior becomes recurrent and stabilized among many of those who engage in it Many of these persons define participation in "blow-jobs" in positive terms, but those sentiments do not arise out of conversations and social interchange among homosexuals This example was brought to my attention by one of the readers for the journal.

Situational Elements in Criminality

Other patterns of offender behavior can be isolated in which situational elements have primacy, so that probing about for genetic factors is unwarranted. For example, consider the report in *Tally's Corner,* where it was noted that the incidence of burglary and other property crimes increased markedly during winter, when Negro construction workers are laid off and turn to crime in order to eke out a living (Liebow 1967, pp. 29–71). There is little in that report to suggest that criminality is a preferred pattern of behavior by these persons, that they are the carriers of an excess of definitions favorable to violation of law, or that their lawbreaking is the outgrowth of a lengthy genetic process of differential socialization. Along the same line, attention ought to be paid to Gould's analysis (1969), which relates the upsurge of car theft and bank robberies over the past several decades to the growing abundance of these "victims." Then, too, consider Camp's research (1967) which shows that bank robberies are usually the work of desperate men. These acts are carried out as a last resort to solve some perceived crisis in the life of the robber, rather than the acts of criminal gangs who make a career of carefully planned heists, or whose lawbreaking has causal antecedents located in criminal associations which occurred years earlier.

Other patterns of offender behavior can be isolated in which situational elements loom large, such as drunk driving and manslaughter. Additionally, Cameron's study (1964) shows that "snitches" or amateur shoplifters are "peripheral" criminals rather than "vocational" offenders. Prior to arrest, they do not think of themselves as thieves, and no clear-cut genetic process emerges in their backgrounds. Some other evidence on juvenile and adult shoplifting supports this characterization of persons who engage in flirtations with deviant acts (Gibbons 1968, pp. 287–94).

Another case of situational factors is Lemert's report (1967) on naïve check forgery, which he contends is the outgrowth of a process of "risk taking." He defines the risk-taking process in the following terms: "This concept refers to situations in which persons who are caught in a network of conflicting claims or values choose not deviant alternatives but rather behavioral situations which carry risks of deviation. Deviation then becomes merely one possible outcome of their actions, but it is not inevitable" (Lemert 1967, p. 11). An example of this process might be the individual who writes a check while drinking, hoping to be able to reach the bank with funds to cover it before it is presented to the bank. Lemert argues that close examination of other kinds of criminality might also show risk-taking elements.[10]

[10] Naive check forgery has been offered both as an illustration of genetic and situa-

Interaction of Genetic and Situational Factors

So far, I have argued that some forms of criminal behavior involve a heavy component of genetic etiology, while situational elements loom large in others. However, there probably are many instances where both genetic factors and situation contingencies are significant. For example, acts of murder appear to be most frequent among those who have grown up in a "subculture of violence," who have been subjected to a number of disorganizing social influences over an extended period of time, and who are disposed to look upon others as potential assailants (see Gibbons 1968, pp. 349–56). At the same time, not all who share such experiences commit acts of homicide. Those who do engage in murder often do so within situations of marital discord or tavern fights, in which a number of provocative moves and countermoves of interactional partners culminate in acts of homicide (see Wolfgang 1958, pp. 264–65). However, it would be incorrect to suppose that many of the participants in these short-lived sets of events intended them to have this criminal outcome, even a minute or so before the killing actually occurred.

This example of homicide can be viewed as a criminological application of the "value-added" framework employed by Smelser (1963) and Lofland and Stark (1965) in the analysis of collective behavior and social movements. As used by Smelser, the "value-added" formulation refers to a series of stages or events in which each must occur according to a particular pattern in order for a certain outcome to be produced. He notes: "Every stage in the value-added process, therefore, is a necessary condition for the appropriate and effective addition of value in the next stage. The sufficient condition for the final production, moreover, is the combination of *every* necessary condition, according to a definite pattern" (Smelser 1963, pp. 13–14).[11]

A value-added conception of homicide would assert that the experience of growing up in a subcultural setting where violence is a common theme is a precondition for violent acts, but that specific instances of aggression and homicide do not occur until other events transpire, such as a marital dispute while drinking. Another application of value-added ideas can be made to forcible rape. The available research evidence (see Gibbons 1968, pp. 384–89) indicates that forcible rapists are usually working-class males

tional processes This case suggests something of the complexity of causal patterns A plausible case can be made holding that initial acts of check writing occur in a risk-taking way. Subsequently, discovery that this activity is a low-risk form of criminality may trigger a genetic process of the kind sketched earlier

[11] Cressey's (1953) analysis of financial trust violation can be seen as a case of value-added theory. He argues that nonsharable problems are important in trust violation, but only those persons with these problems who experience certain other life events become engaged in trust violation

from a social situation where exploitative and aggressive themes regarding females are commonplace. But, only a small number of those males who regard sexual intercourse as something to be done *to* rather than *with* a female, or who express similar attitudes, become involved in forcible rape. One of the most important factors in the value-added process, increasing the likelihood that forcible rape will occur, may be the situational one of sexually provocative interaction between a male and female during an evening of drinking.

In both of these illustrations, genetic processes leading to acquisition of definitions favorable to violation of law are acknowledged to be important. However, not all who exhibit these sentiments engage in deviant acts, so that other causal events must also be involved in lawbreaking

A value-added analysis of criminality would produce a considerably different view of the relationship between genetic and situational factors than the one emphasized by Sutherland. As noted above, he argued that proximate influences of a situational kind were unimportant in criminality compared to conduct definitions acquired from genetic processes I have suggested in the preceding commentary that situational influences may often be the crucial, final element in a value-added process that ends in lawbreaking. Without the presence of the situational correlates, the necessary and sufficient causes of at least some kinds of criminality fail to occur

In my view, there is much to be said for the exploration of such familiar notions as "criminogenic culture" from a value-added position which looks for links between genetic and situational factors For example, it may be that definitions favorable to violation of law in the form of tolerant sentiments toward petty theft are widespread in American society and much less common in some other nation, so that persons in this country commonly do learn these attitudes from some kind of genetic socialization experience. If so, these genetically acquired definitions would account in part for cross-cultural variations in crime rates. They would be etiologically significant, even though additional factors would have to be identified in order to uncover the complete causal pattern producing criminally deviant acts by specific individuals.

SUMMARY

This paper has issued a call for more exploration of situational elements in criminality. I have argued that the lack of progress on etiological questions has been the result of inordinate attention to motivational formulations and genetic processes. Although both genetic and situational factors are implicated in criminality, the thesis here is that the latter may well be more important and more frequently encountered than many criminologists have acknowledged to date. The major tasks for criminological

investigation center about identification of those cases where one or both of these sets of influences are operating, explication of the relationships between these factors, and determination of the relative weight to be assigned to these factors in particular cases of criminality.

Limited space precludes spelling out a detailed inventory of the forms of criminal etiology. Nor at present can I offer a full-blown explication of this kind. But some sense of the directions which ought to be explored is contained in the above discussion of particular cases of lawbreaking. Implicit in that material is the view that situational elements are probably involved in a good many nontrivial instances of criminality, so that it is not just the speeding motorist who responds in a criminal fashion to the inducements of the moment. Thus the prisons may be filled with a large number of persons who became enmeshed in criminality more out of adverse situations than of differential learning.[12]

The general programmatic recommendation of this essay is for more exploration of other possibilities where situational factors are operative Thus close examination might be made of a recent account by Jackson (1969) of the life style of a contemporary Texas "character" (criminal argot for a police character, that is, an offender well-known to law enforcement agencies). This thief is a safe-robber from an upper middle-class background who drifted into deviance after becoming detached from familial ties, rather than being inducted into it through some kind of associational learning. Once involved in the thief life, this offender discovered that he enjoyed stealing and the "wine, women, and song" life style which accompanies it.[13] Another instructive case is Geis's (1968, pp. 103 18) analysis of the occupational cross pressures under which participants in the heavy electrical equipment antitrust cases of 1961 found themselves. These situational inducements to law violation appeared to influence this behavior more than any kind of learning of antisocial sentiments through differential association.

Moving to an area of criminality which has yet to be subjected to intensive investigation, we might wonder what kinds of situational factors might turn up on income-tax evasion. Suppose that income-tax violations were to be studied by some kind of anonymous self-report technique. We

[12] Consider the extreme case of contemporary federal prisons which contain many persons sentenced for draft evasion. Many of these youths were responding to differential association of a reverse form to that visualized by Sutherland. They were exposed to extensive and prolonged moral socialization stressing the wrongfulness of killing and warfare. Surely they would not be in prison if the current societal conditions were somewhat different, and it would be hard to characterize them as "antisocial" persons. Joseph F. Jones brought this point to my attention

[13] I have discussed Jackson's book with a criminologist and friend of mine who has served a long sentence in a California prison He avers that a number of the thieves he knows express positive enjoyment of stealing and the thrills accompanying it once they became enmeshed in that activity

might discover, first, that citizens generally regard petty income-tax evasion as acceptable behavior if one can "get away with it." We might also find that cheating on income-tax returns is class-linked and widespread among those who have opportunities to do so. Laborers and semiskilled workers rarely evade income taxes because their taxes are routinely withheld from their salary checks and they take standard deductions, so that they have little opportunity to file false claims. Persons in higher income brackets are able to falsify their itemized deductions or to withhold payment of tax on hidden forms of income not subject to payroll deduction.

Examples illustrating the need for attention to situational elements in lawbreaking could be enumerated at some length However, systematic attention to etiological theory is required, rather than the multiplication of illustrations. It is hoped that this paper will trigger some movement toward systematic theory building of the kind hinted at above.

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An Alternative Model for Research on Catholic Education[1]

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Most of the research that has been done on Catholic education has utilized survey methods in the study of attitudes toward and effects of Catholic schools; individual Catholic laymen or schoolchildren have been the objects of analysis, and the orientation has been instrumentalist. An alternative model is suggested as a means of answering questions about the parochial school system itself; this orientation is more nomothetic than instrumentalist. The basic hypothesis, rooted in open-systems theory of organization, is that Catholic education arises out of a perception, on the part of the Roman Catholic church, of an environment threatening to itself. This hypothesis is elaborated in terms of four variables: minority position, ethnicity, hostility of environment, and modernization. Demographic and socioeconomic data are used to test the hypotheses, with states as units of analysis; correlation and regression are the analytic techniques. With some modifications, the results of the analysis support the organizational model.

Points of conflict between sectors of society tend to draw the attention of social researchers, one such point of conflict is Catholic education. On the one hand, there is a governmental structure which provides public education and largely excludes church-run schools from a share of tax dollars. On the other hand is the Roman Catholic church, which continues to maintain a parochial school system and to argue for a share of public money to help support it (see Bishops of America 1968). The conflict has had important consequences. Lekachman (1963), for example, points out that federal aid to education was severely curtailed when it came before the Kennedy administration because of the insistence of the Catholic lobby that if aid be given, parochial schools should be included among the recipients. In recent years the conflict has appeared between factions within Roman Catholicism itself; the popular Catholic press has devoted many pages to the debate between those who would abolish parochial schools and those who would retain them.

Because it is an area of conflict, Catholic education has become a focus of sociological research. The purpose of this paper is to point out certain

[1] Of those scholars whose comments and criticisms have helped to strengthen this research, Mayer Zald, J D. Thompson, and Leo Rigsby of Vanderbilt University, and H David Allen of Louisiana State University deserve special mention and grateful acknowledgment

limitations of the model of research that has most frequently been employed in the study of Catholic education and to propose an alternative model, along with some results of one project where this model was used.

Most of the research done thus far (see, for example, Rossi and Rossi 1961; Bressler and Westoff 1963; Dougherty 1965; O'Neill 1968; Elford 1969; Westhues 1969) has utilized survey techniques, and the focus has been on attitudes concerning parochial schools, effects of parochial school attendance, and the characteristics of parochial school products. This is also the style of research of the most massive study of Catholic education that has appeared to date, Andrew Greeley and Peter Rossi's *The Education of Catholic Americans* (1968). Using a national probability sample of American Catholics drawn in 1962, they selected 2,753 individuals for interviews and secured an additional 1,000 self-administered questionnaires from others in the 1962 sample. Questionnaires were also sent to a sample of 1,000 subscribers to *Commonweal* so that comparisons could be made between the liberal Catholic intelligentsia and the general Catholic population in their attitudes toward Catholic education.

The style of research best represented by the Greeley-Rossi study has for its object of analysis the attitudes and characteristics of individual people. Its data are the behavior patterns of Catholic laymen and Catholic schoolchildren. In this way it focuses on the effects of Catholic school attendance on Catholic children and on the effects that the attitudes of lay Catholics can be expected to have on Catholic schools.

For this reason, the principle value of such research is an instrumentalist one: those who control Catholic education can, with these findings in hand, more wisely make decisions concerning the Catholic school system. With accurate knowledge of the kinds of effects parochial schools have had on their pupils and of the extent of support for these schools by their clients, decision makers—not only bishops, religious orders, and legislators, but lay Catholics themselves—can more efficiently decide what action to take concerning Catholic education, given their own particular values. Greeley and Rossi make their instrumentalist orientation explicit when they write: "It seems to us, rather, that the relevant question is how the distinctive system of Catholic schools can make the strongest possible contribution to the health of American society" (1968, p. 239).

The alternative style of research we propose here has for its object of analysis not individual behavior but organizational behavior. Its data are not the effects of Catholic schools or attitudes concerning them but, rather, the conditions under which the Catholic school *system* behaves in different ways. While this second style of research might also have instrumentalist applications, its aim is more purely nomothetic: it seeks to specify the determinants of organizational behavior, without necessarily providing

fuel for the political controversies within the organization itself or between it and other groups in American society.

The most fundamental question of such an alternative style of research is: under what conditions is the Catholic school system weak and under what conditions is it strong? To the extent that it can specify these conditions, research of this second type can predict when the Catholic church will provide its own schools for Catholic children and when it will not. To such an extent, moreover, within the limits of present historical trends, a prediction can be made of the future of Catholic education in the United States.[2]

AN ORGANIZATIONAL MODEL[3]

The object of analysis in the research to be reported here is the Roman Catholic church in the United States. We regard it as an organization subject to the norms of rationality common to all organizations. The underlying theoretical hypothesis used here to explain Catholic church involvement in education is provided by J. D. Thompson (1968, p. 119): "Under norms of rationality, organizations seek to seal off their core technologies from environmental influences." The technology of Roman Catholicism is the set of beliefs and behaviors by means of which the goal of the organization (salvation, heaven, a life pleasing to God) is believed to be achieved. To the extent that the church's environment can interfere with this technology (by, for example, weakening members' commitment to orthodox beliefs), the environment poses a threat to the church; one of the church's responses to this threat is the maintenance of a system of sectarian schools, where the harmful influences of environmental culture can be removed from or reinterpreted within the educational process. This explanation of the origins of parochial schools is suggested by Cross (1965), Greeley and Rossi (1968, p. 75), and Rothman (1963); the last-named remarks: "Given a compulsory public school system, and schools which were either vaguely Protestant or vaguely secular, the faith of Catholic children, it was felt, could only be preserved if the Church developed a mass system of its own" (1963, p. 59).

In the model we propose, the theoretical hypothesis of the environmental threat is operationalized in terms of four research hypotheses. A fifth

[2] Greeley and Rossi (1968, pp 207–26) attempt a prediction of the future of Catholic education, but it cannot be taken as definitive. Because it is based on expressed attitudes of lay Catholics, it assumes an isomorphic relationship between these attitudes and economic and religious behavior, it also assumes that lay Catholics are the only crucial control agent of Catholic education. There is little evidence to support these assumptions.

[3] The relevance of this question to secularization theory is spelled out in Westhues (in press).

hypothesis concerns the density of the Catholic population; this variable is suggested by Greeley and Rossi to explain differential availability of parochial schools (1968, p. 294). These hypotheses are used to explain differences in the extent of church involvement in parochial school education across states. They hypothesize the conditions under which the Roman Catholic church can be expected to set up and maintain a school system of its own. In the paragraphs below are given the hypotheses, along with measurements of the variables within them.

The more the church is in a minority, the greater its involvement in education. Our reasoning here is simply that the more any distinctive group finds itself in a minority, the more it will feel threatened by its environment.[4] In his own distinctive language, Peter Berger offers some theoretical support for this hypothesis: "The reaffirmation of orthodox objectivities in the secularizing-pluralizing situation, then, entails the maintenance of sectarian forms of socio-religious organization. . . . It may be seen in the Catholic case (despite the universalistic, profoundly anti-sectarian character of Catholic ecclesiology), wherever Catholicism seeks to maintain itself in a massively non-Catholic milieu" (1969, p. 164). Minority position is measured here as the percentage of the total population who are members of the Catholic church; data are taken from the *Official Catholic Directory*

The more the environment of the church is composed of religions overtly hostile to Catholicism, the greater the church's involvement in education We would expect greater church involvement in education in states where fundamentalist Protestantism, Mormonism, and similar denominations and sects openly antagonistic to Catholicism dominate than in states dominated by liberal Protestantism. A measure of this variable was obtained from the census taken in 1952 by the National Council of Churches: the percentage of the population of non-Catholic church members over the age of thirteen who belong to the Southern Baptist Convention, American Baptist Convention, Latter Day Saints, Church of the Nazarene, Churches of God (of Indiana and Tennessee), Assemblies of God, or Seventh Day Adventist. A similar measure was obtained for 1926 using data from the federal Census of Religious Bodies for that year[5] In both cases the precise denominations to be included were dictated to some extent by the manner

[4] Terry Clark's research (1968) on community structure, decision making, budget expenditures, and urban renewal across American cities may be considered as supporting this hypothesis Using a method nearly the same as that I employ, he found a positive relationship between percentage Catholic and urban renewal Perhaps this otherwise unexplained finding could be explained by our hypothesis that the less that Catholics are in a minority, the less they feel threatened by their environment and the more willing they are to support secular welfare programs.

[5] In 1926 the demoninations included were the following: the four major Baptist bodies, Churches of Christ, Disciples of Christ, and Latter Day Saints.

in which the censuses categorized data; the measures, however, include the bulk of the church members we wanted to include.

The more membership in the church overlaps with ethnic identity, the greater the church's involvement in education. Oscar Lee (1960) has pointed out the close relationship between religion and ethnicity: "It seems clear that a major function of the churches of ethnic and racial minorities is to serve the religious and social needs of their people. This function is shaped and reinforced by ethnic and racial discrimination, which is a dominant factor influencing every aspect of the lives of the people belonging to these minority groups. Their aspirations are an influential factor in shaping the goals toward which these churches work" (1960, p. 123). If the differences between Catholics and their environment extend beyond doctrine, moral codes, and ritual to encompass a whole ethnic tradition, the church should find its environment all the more threatening and involve itself in education more extensively. A clean measure of ethnicity is impossible to construct across states; church censuses report data on religion but not ethnicity, and the decennial censuses of population up to and including 1950 report data on ethnicity but not religion. The measure we use here is a fraction, the denominator of which is the state's total Catholic population (from the *Official Catholic Directory*) and the numerator the state's foreign-born population (from the U.S. population census) from each of the following countries: Ireland, Germany, French Canada, Italy, Lithuania, Poland, and Czechoslovakia.[6] This measure overrepresents the foreign-born Catholic population, because it includes the foreign-born from these seven countries who are not active Catholics, but there is no reason to think that the error is not randomly distributed and more accurate measurements are simply unavailable.

The more modernized the milieu of the church, the less its involvement in education. The reasoning behind this hypothesis is less obvious than in the case of the preceding hypotheses. Herberg (1955) has argued that in emergent American culture, Protestantism, Catholicism, and Judaism lose their identity as distinct religious orientations and become merely variations on the theme of American religion. Berger (1969) uses the term "subjectivization" to describe the status of religion among people in secularized

[6] Foreign-born from Mexico, Puerto Rico, and other Latin societies are absent from our measure, because Catholic immigrants from the Latin countries have not been known to perceive the church as a source of ethnic identity in the way European immigrants have done (see Greeley and Rossi 1968, pp. 36ff). The nationalities represented by the seven countries in our measure accounted for 72 percent of all Catholic churches using a foreign language in 1916 and 81 percent of the members of those churches (U S Bureau of the Census, *Census of Religious Bodies*, 1916), this was the last year such statistics were computed We thus feel justified in asserting that these countries account for virtually all the foreign-born Catholics we want to include in our measure.

society. Nisbet (1964) discusses the replacement of religion by technology as a source of ethical decision making. The relevance of these and other theorists to our concern here is that we expect that, in areas where these emergent religious trends are embodied, the Catholic church will feel less threatened by its environment and will thus be less willing to maintain a sectarian system of education. Quantitative measurement of such a qualitative variable as modernization in this cultural sense is difficult. Two measures are used here, percentage urban and a summary measure of education, percentage illiterate before 1940 and median years of education after 1940; data are taken from the federal census of population. We admit that there is some discrepancy between the conceptual and operational meanings of the modernization variable, but we do not believe it is so great that the research is invalidated.

Below a certain level of density of the Catholic population, the church's involvement in education will be lower than would otherwise be expected This hypothesis, suggested by Greeley and Rossi, is rooted in the spatial limits within which an organization must act rather than in the notion of the environmental threat. In the model we propose, it has the status of a control variable: the variables in the previous four hypotheses should be able to account for variation in church involvement in education only when there are a sufficient number of Catholics grouped together to make the maintenance of sectarian schools possible. The measure for the density of the Catholic population uses data from the *Official Catholic Directory:* the mean size of parishes in the state.

Five hypotheses have been stated and quantitative measures of the predictor variables defined. The remaining measurement problem, of the dependent variable, is perhaps the most serious. Church involvement in education is best measured as the percentage of Catholic schoolchildren attending Catholic schools. Providing every Catholic child with a Catholic education was set forth by the Councils of Baltimore early in the history of American Catholicism as the ideal; variation in the extent to which this ideal has been realized constitutes differential involvement of the church in education across states. Unfortunately, the *Official Catholic Directory* provides no data on the percentage of Catholic children attending Catholic schools; these data are collected only sporadically by a few dioceses.[7] The measure we use here for 1950 is simply the number of children in parochial grade schools divided by the total Catholic population; the measure of church involvement in high school education is the same, except that the numerator in this case is the number of students in

[7] Requests for this information were sent in 1968 to a representative sample of twenty-one dioceses in the country; the eighteen replies yielded two fairly reliable estimates and sixteen admissions that no one in the diocese knew this information

parochial and diocesan high schools. The validity of these measurements depends upon three assumptions, the first of which is that only Catholic children attend Catholic schools. Familiarity with Catholic grade and high schools across the country leads one to conclude that more than 95 percent of the students are typically Catholic; if the church is providing some non-Catholic children with a Catholic education, the church's motivation of explicit or implicit proselytization would not seem to spring from a different source than its attempt to retain the commitment of those already in the church. Hence, whatever small amount of error is introduced by this assumption could probably be explained by these same hypotheses. A second assumption is that the percentage of Catholic children attending school at all is constant across states. This assumption is less warranted earlier in the century than today: the mean percentage across states of children in the grade-school age range attending school rose from 90.7 in 1920 to 97.5 in 1960, while the standard deviation of this percentage declined from 5.6 in 1920 to 0 8 in 1960 (based on data from the U.S. population census). We expect that the pattern is the same for the Catholic population; if this is so, the error introduced in this measure for the later years is minimal. The third assumption of the measure of church involvement in education we employ is that the age distributions of Catholics across states are roughly equivalent. We know of no evidence that would either confirm or contradict this assumption. Fortunately, however, the federal census of religious bodies reported in 1926 the percentage of the Catholic population less than thirteen years of age in each state. By using the Catholic population less than thirteen years of age as the denominator in the fraction defined above, we admit as error only variation across states in the percentage of the Catholic population less than thirteen years of age who are less than six years of age. If a test of the hypotheses on data of 1926 yields the same pattern of results as a test on data from 1950, our confidence in the greater accuracy of the 1926 measure will make us more confident of the cruder measure in 1950 and lend support to our assumption of roughly equivalent age distributions.

DATA ANALYSIS

Table 1 presents means and standard deviations across states of all the variables we have constructed for two points in time, 1926 and 1950. No measure of church involvement in high school education could be constructed for 1926, because the *Official Catholic Directory* had not yet begun to report data on Catholic high schools in that year. It should be noted that it is impossible to compare the extent of church involvement in education between 1926 and 1950, or between other points in time, with the measures available, because this would require that the assumptions

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Year	Variable	Mean	SD
[illegible]	Percentage Catholic	[illegible]	[illegible]
[illegible]	Percentage Catholic	[illegible]	[illegible]
[illegible]	Hostility of environment	[illegible]	[illegible]
[illegible]	Hostility of environment	[illegible]	[illegible]
[illegible]	Ethnicity	[illegible]	[illegible]
[illegible]	Ethnicity	[illegible]	[illegible]
1920	Percentage illiterate	[illegible]	[illegible]
[illegible]	Median years of education	[illegible]	[illegible]
[illegible]	Percentage urban	[illegible]	[illegible]
[illegible]	Percentage urban	[illegible]	[illegible]
[illegible]	Mean parish size	[illegible]	[illegible]
[illegible]	Mean parish size	[illegible]	[illegible]
1926	Grade school involvement	40 8	16 9
1950	Grade school involvement	10 5	4 3
1950	High school involvement	1 51	0 95

* The N for these and subsequent computations is 47, for all states on the continent except Nevada, which was excluded because of unavailability of data in the Catholic sources

(equivalent age distributions, for example) made for the validity of these measures across states also be made across points in time; this we do not think is justifiable. Thus, the purpose of presenting means for 1926 and 1950 in table 1 is only so that analyses can be performed at two discrete points in time.

In table 2 are given block correlations of predictor and dependent vari-

TABLE 2

Zero-Order Correlation Matrices of Five Predictor Variables and the Dependent Variables at Two Points in Time, 1926 and 1950

Variable	X_1	X_2	X_3	X_4	X_5	Y	Z
	For Dependent Variable Measured in 1926						
X_1, percentage Catholic, 1926	1 000	..	..	...	..	.	
X_2, hostility of environment, 1926	— 584	1 000	...	.	..	..	
X_3, ethnicity, 1920	— 013	— 210	1 000	.	..	...	.
X_4, percentage illiterate, 1920	— 149	539	— 508	1 000	..	..	
X_5, percentage urban, 1920	637	— 508	479	— 371	1 000	...	
Y, grade school involvement, 1926	— 394	112	143	153	— 006	1 000	
	For Dependent Variable Measured in 1950						
X_1, percentage Catholic, 1950	1 000	..	..	...	..	.	
X_2, hostility of environment, 1952	— 487	1.000	..	.	..		
X_3, ethnicity, 1950	103	—.259	1 000	.	..	..	
X_4, median years of education, 1950	.203	— 181	457	1 000	..	..	..
X_5, percentage urban, 1950	642	—.382	.457	.442	1 000	..	..
Y, grade school involvement, 1950	— 577	169	145	— 529	—.323	1 000	.
Z, high school involvement, 1950	—.490	214	— 060	—.374	— 272	611	1 000

two points in time. Mean parish size is omitted from ... ause it will be analyzed separately as a control variable. ... served that, in zero-order correlation, all the relationships at ... nts in time are as predicted for grade school involvement: negative ... n with percentage Catholic, percentage urban, and median years ... tion, and positive association with hostility of environment, ..., and percentage illiterate. In the case of high school involvement, ... tionships are as predicted, except for the ethnicity variable, with ... it has a small negative association.

... bles 3 and 4 present zero-order correlations, partial correlations (four

TABLE 3

CORRELATIONS AND THE REGRESSION MODEL FOR THE DEPENDENT VARIABLE CHURCH INVOLVEMENT IN PAROCHIAL GRADE SCHOOLS, 1926 (Y)

Predictor Variable	Zero-Order Correlation	Partial Correlation	Standardized β-Weight
X_1, percentage Catholic, 1926	— 394	— 546	— 858
X_2, hostility of environment, 1926	112	— 283	— 346
X_3, ethnicity, 1920	143	026	024
X_4, percentage illiterate, 1920	153	353	411
X_5, percentage urban, 1920	— 006	350	507

$$Y^* = 41\ 319 - \underset{(3103)}{1\ 2953X_1} - \underset{(1422)}{2682X_2} + \underset{(2121)}{0359X_3} + \underset{(4573)}{1\ 1055X_4} + \underset{(1673)}{3996X_5}$$

NOTE —R = 602, variance explained = 36 2 percent
* Regression coefficients in this equation are unstandardized, standard-error terms in parentheses

TABLE 4

CORRELATIONS AND THE REGRESSION MODEL FOR THE DEPENDENT VARIABLE CHURCH INVOLVEMENT IN PAROCHIAL GRADE SCHOOLS, 1950 (Y)

Predictor Variable	Zero-Order Correlation	Partial Correlation	Standardized β-Weight
X_1, percentage Catholic, 1950	— 577	— 609	— 673
X_2, hostility of environment, 1952	169	— 170	— 121
X_3, ethnicity, 1950	145	332	256
X_4, median years of education, 1950	— 529	— 651	— 574
X_5, percentage urban, 1950	— 323	207	197

$$Y^* = 33\ 089 - \underset{(0423)}{2077X_1} - \underset{(0201)}{0223X_2} + \underset{(0725)}{1636X_3} - \underset{(.4476)}{2\ 4568X_4} + \underset{(0412)}{0558X_5}$$

NOTE - R = 803, variance explained = 64 5 percent
* Regression coefficients in this equation are unstandardized, standard error terms in parentheses

predictor variables being controlled), and standardized partial regression coefficients, along with values of R and R^2 and the unstandardized regression equation, for the models predicting church involvement in parochial grade school education in 1926 and 1950. In both models, percentage Catholic is the strongest statistical predictor, and the relationship is in the

TABLE 1

MEANS AND STANDARD DEVIATIONS OF PREDICTOR AND DEPENDENT VARIABLES*

Date	Variable	Mean	SD
1926	Percentage Catholic	14 6	11.2
1950	Percentage Catholic	18 1	13 9
1926	Hostility of environment	30.9	22 2
1952	Hostility of environment	29 1	23 3
1920	Ethnicity	31.0	14 0
1950	Ethnicity	12.7	6 6
1920	Percentage illiterate, ages 25+	7.8	6 4
1950	Median years of education	9.4	1 0
1920	Percentage urban	42 7	21.2
1950	Percentage urban	51 4	15 6
1920	Mean parish size	1,313 8	731 8
1950	Mean parish size	1,427.9	845 6
1926	Grade school involvement	40 8	16 9
1950	Grade school involvement	10.5	4 3
1950	High school involvement	1 51	0 95

* The N for these and subsequent computations is 47, for all states on the continent except Nevada, which was excluded because of unavailability of data in the Catholic sources

(equivalent age distributions, for example) made for the validity of these measures across states also be made across points in time; this we do not think is justifiable. Thus, the purpose of presenting means for 1926 and 1950 in table 1 is only so that analyses can be performed at two discrete points in time.

In table 2 are given block correlations of predictor and dependent vari-

TABLE 2

ZERO-ORDER CORRELATION MATRICES OF FIVE PREDICTOR VARIABLES AND THE DEPENDENT VARIABLES AT TWO POINTS IN TIME, 1926 AND 1950

Variable	X_1	X_2	X_3	X_4	X_5	Y	Z
	For Dependent Variable Measured in 1926						
X_1, percentage Catholic, 1926	1 000	...	...		...	..	
X_2, hostility of environment, 1926	— 584	1 000	...	...	..	...	
X_3, ethnicity, 1920	— 013	— 210	1.000	..		...	.
X_4, percentage illiterate, 1920	— 149	.539	— 508	1 000	. .	..	.
X_5, percentage urban, 1920	637	— 508	479	— 371	1 000	. .	
Y, grade school involvement, 1926	— 394	112	143	153	— 006	1.000	
	For Dependent Variable Measured in 1950						
X_1, percentage Catholic, 1950	1.000	...	.	...	...	...	. .
X_2, hostility of environment, 1952	—.487	1 000	.	...	...	.	.
X_3, ethnicity, 1950	103	— 259	1 000	..	...	...	.
X_4, median years of education, 1950	203	— 181	.457	1 000	. .	...	. .
X_5, percentage urban, 1950	.642	—.382	.457	442	1 000	...	.
Y, grade school involvement, 1950	— 577	169	.145	— 529	—.323	1 000	.
Z, high school involvement, 1950	— 490	.214	— 060	— 374	— 272	611	1 000

ables at each of the two points in time. Mean parish size is omitted from these matrices because it will be analyzed separately as a control variable. It will be observed that, in zero-order correlation, all the relationships at both points in time are as predicted for grade school involvement: negative association with percentage Catholic, percentage urban, and median years of education, and positive association with hostility of environment, ethnicity, and percentage illiterate. In the case of high school involvement, the relationships are as predicted, except for the ethnicity variable, with which it has a small negative association.

Tables 3 and 4 present zero-order correlations, partial correlations (four

TABLE 3

CORRELATIONS AND THE REGRESSION MODEL FOR THE DEPENDENT VARIABLE CHURCH INVOLVEMENT IN PAROCHIAL GRADE SCHOOLS, 1926 (Y)

Predictor Variable	Zero-Order Correlation	Partial Correlation	Standardized β-Weight
X_1, percentage Catholic, 1926	− 394	− 546	− 858
X_2, hostility of environment, 1926	112	− 283	− 346
X_3, ethnicity, 1920	143	026	.024
X_4, percentage illiterate, 1920	153	353	411
X_5, percentage urban, 1920	− 006	350	507

$$Y^* = 41\ 319 - \underset{(3103)}{1\ 2953X_1} - \underset{(1422)}{2682X_2} + \underset{(2121)}{.0359X_3} + \underset{(.4573)}{1\ 1055X_4} + \underset{(1673)}{3996X_5}$$

NOTE.—R = 602, variance explained = 36 2 percent
* Regression coefficients in this equation are unstandardized; standard-error terms in parentheses

TABLE 4

CORRELATIONS AND THE REGRESSION MODEL FOR THE DEPENDENT VARIABLE CHURCH INVOLVEMENT IN PAROCHIAL GRADE SCHOOLS, 1950 (Y)

Predictor Variable	Zero-Order Correlation	Partial Correlation	Standardized β-Weight
X_1, percentage Catholic, 1950	− 577	− 609	−.673
X_2, hostility of environment, 1952	169	− 170	−.121
X_3, ethnicity, 1950	.145	332	.256
X_4, median years of education, 1950	− 529	− 651	− 574
X_5, percentage urban, 1950	− 323	.207	197

$$Y^* = 33\ 089 - \underset{(0423)}{2077X_1} - \underset{(0201)}{0223X_2} + \underset{(0725)}{1636X_3} - \underset{(4476)}{2\ 4568X_4} + \underset{(0412)}{0558X_5}$$

NOTE.—R = 803, variance explained = 64.5 percent
* Regression coefficients in this equation are unstandardized, standard error terms in parentheses

predictor variables being controlled), and standardized partial regression coefficients, along with values of R and R^2 and the unstandardized regression equation, for the models predicting church involvement in parochial grade school education in 1926 and 1950. In both models, percentage Catholic is the strongest statistical predictor, and the relationship is in the

direction hypothesized. With the other variables controlled, the measure of hostility of environment is found to be weakly related to involvement in grade schools but in the direction opposite of that hypothesized: hostility of environment, as we have measured it, does not exert an independent effect to make church involvement in elementary education greater. The effect of ethnicity is in the hypothesized direction, though the relationship is small enough to be inconsequential in 1926. Percentage illiterate is positively related to church involvement in 1926, and median years of education is negatively related to church involvement in 1950: in both equations our hypothesis of a negative association between the education dimension of modernization and involvement of the church in elementary schools receives support. Such is not the case, however, with the urbanization dimension of modernization, which reverses sign in the regression equation and is positively associated with the dependent variable The percentage of variation explained is much greater in 1950 than in 1926, due statistically to the stronger relationships at the later point in time between the dependent variable and the measures of minority position, ethnicity, and the education dimension of modernization.

Table 5 presents for 1950 the same correlation and regression model as

TABLE 5

CORRELATIONS AND THE REGRESSION MODEL FOR THE DEPENDENT VARIABLE CHURCH INVOLVEMENT IN PAROCHIAL AND DIOCESAN HIGH SCHOOLS, 1950 (Z)

Predictor Variable	Zero-Order Correlation	Partial Correlation	Standardized β-Weight
X_1, percentage Catholic, 1950	— 490	—.465	— 621
X_2, hostility of environment, 1952	214	— 059	— 053
X_3, ethnicity, 1950	— 060	—.038	— 035
X_4, median years of education, 1950	— 374	— 384	—.376
X_5, percentage urban, 1950	— 272	.218	289

$$Z^* = 4\,870 - .0426X_1 - 0023X_2 - 0053X_3 - 3576X_4 + 0177X_5$$
$$\qquad\quad (0127) \quad (0060) \quad (0218) \quad (1343) \quad (0124)$$

NOTE —R = 598, variance explained = 35 7 percent
* Regression coefficients in this equation are unstandardized, standard-error terms in parentheses

in table 4, but with church involvement in parochial and diocesan high schools as the dependent variable. Once again, the measures of minority position and the education dimension of modernization are the strongest statistical predictors, and the results support our hypotheses with regard to these two variables. The effects of hostility of environment and ethnicity are virtually nonexistent, but in a direction opposite to that hypothesized. Percentage urban, as in the case of grade school involvement, is found to be positively associated with the dependent variable. The percentage of variance explained is about what it was for grade school involvement in 1926.

In order to observe the effect of the measure of density of the Catholic population—mean parish size—the case base of forty-seven states was divided into two subsamples in each of the three regression equations, and the mean residual error of those states below a given level of mean parish size (700 in 1950, 400 in 1926) was compared with the mean residual error of those states having a mean parish size above that level. The results are presented in table 6. For our hypothesis to receive support, it is necessary

TABLE 6

MEAN UNSIGNED RESIDUALS FROM THE REGRESSION MODELS FOR SUBSAMPLES OF STATES ABOVE AND BELOW DESIGNATED LEVELS OF MEAN PARISH SIZE

Regression Equation for Dependent Variable	Subsample	N (States)	Mean Unsigned Residual
Church involvement in grade schools, 1926 . . .	States having mean parish size ≤ 400	12	9 310
	States having mean parish size > 400	35	10 991
Church involvement in grade schools, 1950 . .	States having mean parish size ≤ 700	10	1 794
	States having mean parish size > 700	37	1 996
Church involvement in high schools, 1950 . . .	States having mean parish size ≤ 700	10	0 990
	States having mean parish size > 700	37	0 535

that the mean residual error of the low-density states be larger than the mean residual error of the high-density states. It is only for high schools in 1950 that this is the case.[8] The hypothesis that low density is associated with low church involvement receives no support in the analysis of parochial grade schools in either 1926 or 1950.[9]

DISCUSSION AND IMPLICATIONS

The above analyses have given clear support for our hypotheses of a positive association between church involvement in education and minority position and a negative association between church involvement and cultural modernization as it is measured by educational level of the population. Our hypothesis of a positive association between a hostile non-Catholic environ-

[8] Following are states having a mean parish size of less than 700 in 1950: North Carolina, South Carolina, Tennessee, Alabama, Iowa, North Dakota, South Dakota, Nebraska, Arkansas, and Idaho. States in 1926 having a mean parish size of less than 400 are all of those just mentioned except Tennessee and Alabama and, in addition, Virginia, Georgia, Mississippi, and Oklahoma.

[9] The larger project from which this analysis is taken (Westhues 1970) tested the same regression models on data from 1906, 1916, 1936, 1962, and 1968. Not only are the results presented here confirmed by analyses at the other points in time, but an analysis of residuals from the models having grade school involvement as dependent variable shows no support for the density hypothesis after 1906.

ment and church involvement has not been supported. Table 2 shows the strong positive association between the hostility of environment variable and minority position; the findings indicate that minority position is sufficient to explain the greater involvement of the church in education without the predicted independent effect of hostility of environment. The ethnicity variable did not show consistent or strong effects in the direction hypothesized, but the amount of error in our measure of ethnicity makes us reluctant to discount its effects completely. Regarding population density, we interpret our results to indicate that its effect is minimal on grade school involvement in cross-state analyses of recent decades.[10] We suspect that it is because percentage urban is also a measure of density that the predicted relationship between percentage urban and the dependent variables was not supported by the data. In summary, we conclude from the research that minority position and the educational level of the population are important predictors of the extent of church involvement in education, that a non-Catholic environment overtly hostile to Catholicism does not have the hypothesized independent effect, that ethnicity probably has some effect that cannot be determined by this research, and that the effect of density of the Catholic population is inconsequential for grade school involvement in recent years but is of some importance for high school involvement.

The fact that these conclusions are supported by the analyses at both points in time lends support to the assumptions upon which the validity of the 1950 measures of the dependent variables is based. We suspect that less variance is explained in the 1926 model because of greater error introduced not by variations in age distributions but by variation in the percentage of Catholic children of grade school age attending school at all.[11] The smaller percentage of variance explained in the model for high schools in 1950 we attribute to the smaller size of this institution, which permits greater error.

It is important to point out that this research represents one preliminary example of the application of an organizational model to the study of

[10] In a supplementary analysis of data, mean parish size was added to the linear regression model for extent of church involvement in grade schools in 1950 When this variable was added, all other relationships remained as before, and mean parish size had an independent negative effect on the dependent variable This suggests that in areas of high concentration of Catholics, the sheer appearance and size of the schools give the impression to the church that it is serving a higher percentage of Catholic children than it is actually serving; thus, in such areas the church is less involved in education than would otherwise be expected.

[11] The validity of my research is further supported by the fact that both in 1950 and in 1962 the ordering of the four regions of the country (South, North Central, Northeast, and West) is the same when my measures are used as when a national probability sample of Catholics is used, as shown in the Greeley-Rossi study (1968, p 294).

Catholic education. Two major limitations of the present study should be pointed out. First is the fact that the correlation-regression model employed cannot give definitive support to the implicitly causal statements of the hypotheses. Second is the use of demographic and socioeconomic data with states as units of analysis, which necessitates construction of highly abstracted variables and consequent inability to specify the mechanisms by which the relationships suggested take place.

These limitations combine with the present findings to suggest first-hand studies of church organizations from this same framework and with specific elaborations of these same hypotheses. It may be, for example, that Catholic education is less extensive in situations of low ethnicity, high percentage Catholic, and high level of modernization because Catholics give less money to the church in these situations than in situations of opposite characteristics. The present study could not test this hypothesis because of the unavailability, on a national scale, of economic or budgetary data on the church. Smaller-scale studies could include this and other variables (for example, diocesan decision-making structure) in a further elaboration of this design.

Because of these limitations, the research is similarly unable to yield a definitive prediction of the future of Catholic education. One could argue, for instance, that if the level of modernization of the population continues to rise, Catholic church involvement in education will decline in the future This argument, however, rests on a number of assumptions—among them, an exhaustive measure of cultural modernization and a basis for causal inference—which have not been justified in this project. Such a prediction is better regarded as a highly tentative conclusion of this research and one that could be made more confidently after additional studies of this kind are done.

In contrast to the dominant style of research in the study of Catholic education, alternative models such as the one employed in this study provide a means of beginning to develop a set of empirically supported propositions that explain differential levels of church involvement in education. The research reported here gives tentative support for a few such propositions

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A Research Note: A Social Structural Model for the Socioeconomic Career[1]

David L. Featherman
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Among a subset of the white male metropolitan population in the years 1957–67, the process of socioeconomic achievement is examined. These longitudinal data on occupational and economic attainment provide comparisons between synthetic cohort and real cohort models of achievement through the middle of the work career. Structural models of career achievement which fail to represent the "historical" quality of work careers are incomplete. In more elaborated causal models of the process of achievement, the roles of paternal occupation, respondent's education, and prior occupational and income attainments are summarized. Fuller details of the data from which this note was drawn appear in Featherman (1969, 1971).

This research summary describes the process whereby a subset of the white male population attains educational, occupational, and economic statuses during the early years of its work career. It probes the dynamic nature or permeability (Svalastoga 1965) of the stratification system, whose "basis and very essence consist in an unequal distribution of rights and privileges, duties and responsibilities, social values and privations, social power and influences among members of a society" (Sorokin 1964, p 11). We understand this process of socioeconomic differentiation and stratification to hinge heavily upon the attainment of occupational statuses, as argued by Blau and Duncan (1967, pp. 5–7); moreover, we accept Blau and Duncan's basic model of occupational achievement (1967, p. 170) as operationally valid for contemporary American society.

Previous empirical assessments of the permeability of the American socioeconomic structure have been limited either by restricted samples (Lipset and Bendix 1964) or by cross-sectional designs (Blau and Duncan 1967). While the data reported here are less than ideal for the establishment of parameters of inter- and intragenerational status attainment in the general population, they do illustrate the process of stratification for a large category of the adult male population in the years 1957–67.

[1] This research was supported by the College of Agricultural and Life Sciences, University of Wisconsin, Madison, and by a grant from the Manpower Administration of the U.S. Department of Labor, grant no. 91-24-69-13, pursuant to the provisions of the Manpower Development and Training Act of 1962.

DATA

Socioeconomic data were generated from those panels of the Princeton Fertility Study (PFS) in 1957, 1960, and the interval 1963–67. Details of sampling and follow-up procedures appear elsewhere (Westoff et al. 1961; Westoff, Potter, and Sagi 1963; Bumpass and Westoff 1970), but the original target population was white couples who resided in seven of the largest SMAs (Standard Metropolitan Areas) and whose second child had been born shortly before the initial interview in 1957. The stratified random sampling design produced 1,165 couples for the original panel; of these, 715 remained in the sample over the restudy period, and they comprise the data for the current investigation.[2]

While the data were collected initially to study the fertility of second-parity metropolitan families, they are useful for the decomposition of the status attainment process for an important segment of the white population. Indeed, one purpose of the PFS investigation was to probe the relationship between fertility and social mobility (Westoff et al. 1961, pp. 6–7). Still, the restrictiveness of actual population coverage and the lack of true birth cohorts would limit our confidence in generalizing the findings too broadly. Having issued the caveat, however, we note that the data generate estimates of the process of stratification not unlike those from more representative cross sections of the population in a similar age range (Duncan, Featherman, and Duncan 1968, pp. 182–98; Featherman 1969). Therefore, we assume that our findings apply to the white metropolitan male population which was about thirty years of age in 1957.

MODELS USING SYNTHETIC AND REAL CAREER DATA

How might we represent the process of socioeconomic achievement through the middle years of the work career? Blau and Duncan have suggested (rather as a heuristic exercise) a path diagram of occupational achievement for a synthetic cohort of nonfarm males from their 1962 "Occupational Changes in a Generation" (OCG) data (Blau and Duncan 1967, fig. 5.2). Through procedures of averaging the corresponding correlation coefficients between social background and achieved-status variables for four ten-year age cohorts and of "borrowing" estimates of ten-year inter-occupational correlations from other data sets, several models of career achievement were produced. One empirically plausible OCG model appears here in truncated form (from data for three age cohorts) as figure 1 and

[2] Of the 1,165 original cases, 814 remained in the data over the ten-year follow-up period The 715 cases in our study population represent the subset of the 814 who also responded to supplementary questionnaires in 1957. In extensive analysis of the effects of ineligibility and case loss on the work reported here, no substantial modifications of the conclusions based on the 715 remaining cases were required (Featherman 1969)

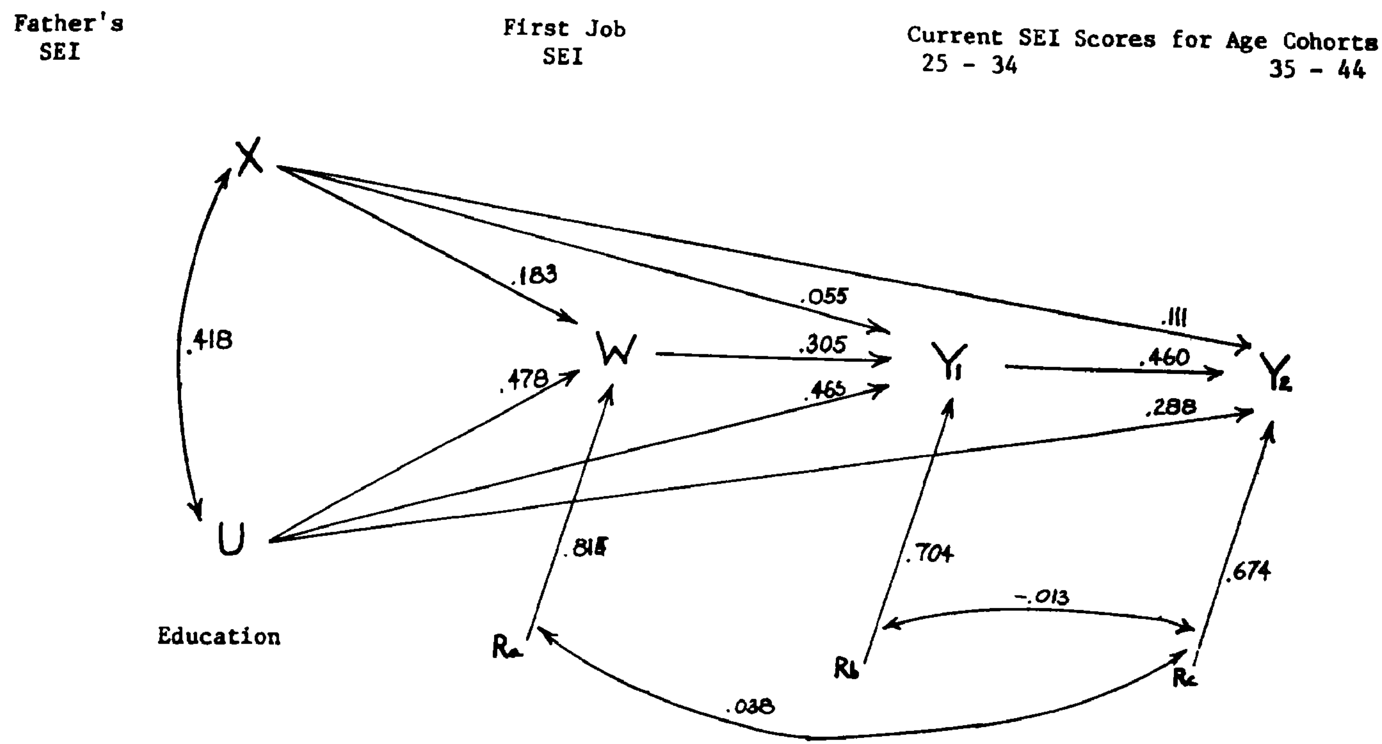


FIG 1—Synthetic cohort model from OCG nonfarm data (Source Blau and Duncan 1967, fig 5 2)

illustrates a first approximation to a more complete model of socioeconomic achievement through the middle years of the work career.

With all occupation variables coded in Duncan SEI scores (Reiss, Duncan, Hatt, and North 1961, pp. 109–38) and education represented by years of formal schooling, figure 1 incorporates the causal assumptions that (*a*) father's occupation and son's education are correlated, predetermined variables, (*b*) son's first, full-time job after completing school and his occupation between ages twenty-five and thirty-four are influenced by all antecedent variables included in the model plus residual effects and error, and (*c*) occupational status some ten years later (ages 35–44) is a function of paternal status or social background, all prior achieved statuses except first job, and the residual term. As such, figure 1 comprises what we might call a modified causal chain, since variable Y_2 is not affected directly by variable W, and both Y_1 and Y_2 are dependent upon X and U in addition to the immediately antecedent variable.

We are not now concerned with the substantive interpretation of the path coefficients in figure 1, as this is rendered elsewhere (Blau and Duncan 1967, pp. 183–88). Rather the issue is the adequacy of this and similar models of the process of career achievement over the period of the life cycle covered by our actual longitudinal data. Note that one implication of figure 1 as drawn is that the occupational career has no "history," namely, that Y_2 is a function of Y_1, the more recently antecedent measured status, but not of W, a status more distantly removed from current achievement. The correlations between the residual paths R_a, R_b, and R_c, while small in magnitude, indicate that the causal assumptions may be inaccurate. Blau and Duncan (1967, p. 186) note that their synthetic cohort model is not able to reproduce the observed correlations, suggesting that the model itself may be defective and that the procedures of averaging cohort data and "borrowing" correlations also may have introduced errors into the estimates.

But what if the same causal assumptions were applied to real as opposed to synthetic career data? Does a modified causal chain, corresponding to figure 1, fit the set of observed correlations in the PFS data? From the coefficients of correlation in table 1 for the PFS sample, the path diagram in figure 2 was computed.

While figure 2 is structurally the same as figure 1, there are notable differences in the population from which the OCG and the PFS samples were drawn, in the definitions of variables, and the scales used to code occupational data. In the PFS data, occupation at marriage is not equivalent to first job, but marriage occurred sufficiently early in the work career (mean age of marriage was 21.8) that the variable's use is not vitiated. Also, the mean age of men at Panel I in 1957 (about thirty years) would place them in the twenty-five to thirty-four-year age

TABLE 1

CORRELATION COEFFICIENTS FOR BACKGROUND AND SOCIOECONOMIC ACHIEVEMENT VARIABLES FOR PRINCETON SUBSET, 1957–67

VARIABLES	VARIABLES									
	X	U	W	IM	Y_1	I_1	Y_2	I_2	Y_3	I_3
Father's Occ-NORC (X)	...	.339	290	095	330	.151	.300	.205	.341	.239
Education (U)	...	...	596	144	636	325	650	.443	.599	478
Occ-NORC, Marr. (W)	...	...	...	221	729	367	643	445	578	.422
Income, Marr. (IM)	...	...	...	...	109	343	078	241	088	195
Occ-NORC, I (Y_1)	...	...	...	...	...	.338	.775	.444	.688	.470
Income, I (I_1)	...	...	...	...	...	...	315	622	.271	.404
Occ-NORC, II (Y_2)	...	...	...	...	...	...	...	444	737	470
Income, II (I_2)	...	...	...	...	...	...	...	...	400	.522
Occ-NORC, III (Y_3)	...	...	...	...	...	...	...	...	...	.514
Income, III (I_3)	...	...	...	...	...	...	...	...	...	...
Means	6 39	12 96	6 60	5,096 25	7 01	6,407 49	7 10	8,279 25	7 32	12,821.86
SD	2.29	2 69	2 22	2,069.43	2 11	3,828 83	2 04	4,993.59	2.10	6,670 14

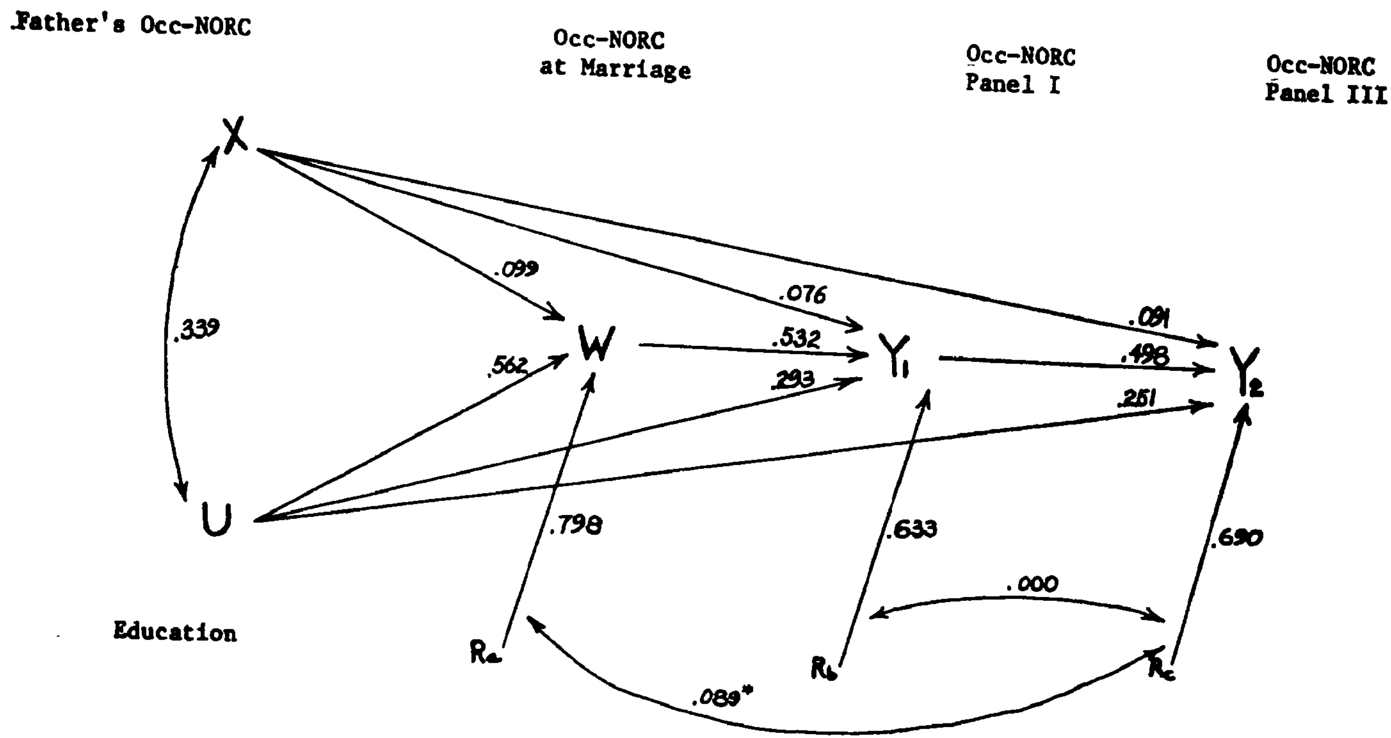


FIG. 2.—Model of occupational achievement at Panels I and III for Princeton subset. * = significant at $\alpha = .05$.

interval, and that of men at Panel III (about thirty-eight years of age) would place them in the thirty-five to forty-four-year age interval. Thus W and Y_1 of figures 1 and 2, and Y_2 in figure 1 and Y_3 in figure 2, would be measures of occupational achievement occurring at roughly comparable points in the life cycle. All occupational responses in the PFS data were coded into prestige score intervals (NORC 1947) rather than Duncan SEI values by the fertility investigators, although the socioeconomic scale and the prestige scales correlate in the range of .86–.91 (Reiss et al. 1961; Duncan et al. 1968, pp. 59–63). These sources of slippage between the OCG and the PFS data sets render any one-to-one comparisons of coefficients in figures 1 and 2 ambiguous, but they do not prevent our answering the question of the adequacy of a model like that proposed (provisionally) by Blau and Duncan for real career data.

In figure 2 (as in figure 1) we observe a positive correlation, r_{ac}. This results from the inequality between the observed correlation r_{WY_3} and that expected from the model

$$\begin{aligned} r_{WY_3} &= P_{Y_3X}\, r_{XW} + P_{Y_3Y_1}\, r_{Y_1W} + P_{Y_3U}\, r_{UW} \\ &= .026 + .363 + .150 \\ .578 &\neq .539. \end{aligned}$$

We might argue that this small discrepancy arises from correlated errors in measurement or reporting of the occupational statuses, owing to the use of the same question in successive interviews. Alternatively, we could argue that the model is correctly specified and that the partial correlation r_{ac} would be zero had we observed the population parameter rather than a sample estimate; however, $r_{ac} = .089$ is significant at $\alpha = .05$ (although it is nonsignificant at $\alpha = .01$). Finally, we might claim that the structural model is inaccurate, because modified causal chains such as figures 1 and 2 misspecify the associations between statuses more than one measurement point apart in the life cycle, that is, W and Y_3. We lack the full capacity to adjudicate among these alternative explanations, although the third is consistent with Hodge's (1966) observation that simple causal chains (first-order Markov models) are not completely adequate for mobility processes such as those considered here. We tend to accept this latter interpretation for the PFS data. Our argument is based on a view of occupational careers which incorporates the possibility that a man's status at Panel III is affected additively and separately by his former status at Panel I as well as that at Panel II.

Since figure 2 is likely to be a misspecification of the process of career achievement, we will not interpret its substance further. We do note that the substantive interpretations of the process of achievement provided by figures 1 and 2 are not too divergent (Featherman 1969, pp. 124–27), which implies that the OCG synthetic cohort data do not grossly mis-

represent the experience of some real cohort, even though the model applied to these data is apparently inaccurate.[3]

SUMMARY OF A MORE ELABORATED MODEL OF SOCIOECONOMIC ACHIEVEMENT

Having asserted that models of occupational attainment over the life cycle should represent any historical effects in the status attainment process for a cohort, we compute the partial regression coefficients in standard form for the four sets of occupational and income dependent variables in the columns of table 2.[4] Income in these data refers to a man's salaries and wages for the year preceding the interview. We view income to be a product of the occupational status and both to be the causal consequences of prior occupational and economic statuses, educational attainment, and social origins (father's occupation). Hence, each dependent variable in table 2 is part of a fully recursive model where paternal occupation and son's education are correlated and, for our current purposes, assumed to be predetermined.

From table 2 we derive an interpretation of the role of social origins and education in the process of socioeconomic achievement which concurs substantially with the conclusions of Blau and Duncan (1967), although our panel design sheds some new light on the total process. The primary effect of social origins is indirect, through the correlation of paternal status and son's education and the rather substantial direct effects of the latter (net of the former) on subsequent socioeconomic statuses.[5] For example, at the time of marriage the direct effect of paternal occupation on that of the son is about one-third of the total relationship between X and W, while at the end of the restudy period (some fifteen years later) the direct effect is only one-fourth of the total correlation. However, we note that at the same time there is not a monotonic decline in the size of

[3] Again, the Blau and Duncan computations were offered by the authors as a heuristic exercise, with the model's structure being dictated in part by the data available to them. Their skepticism about the validity of their model implies that had more intragenerational correlations been available to them, they might have chosen to represent the process of career achievement differently.

[4] Table 2 summarizes the processes of stratification for the PFS cohort Detailed decompositions of this table for the entire cohort (Featherman 1969) and for residential subpopulations (Featherman 1971) appear elsewhere, along with comparisons of unstandardized coefficients.

[5] Other sources examine in detail the relationship between three family-of-socialization variables (paternal occupation, number of siblings, and extent of farm or rural rearing) on educational, occupational, and economic achievements of the PFS men In all instances social background affects the course of the career most heavily through educational achievement. Only paternal occupation bears any direct effect on socioeconomic achievements after education has been controlled statistically (Featherman 1969, 1971)

TABLE 2

PARTIAL REGRESSION COEFFICIENTS IN STANDARD FORM FOR OCCUPATIONAL AND ECONOMIC ACHIEVEMENT AT FOUR POINTS IN LIFE CYCLE ON BACKGROUND AND PREVIOUS STATUSES FOR PRINCETON SUBSET, 1957–67

INDEPENDENT VARIABLES	DEPENDENT VARIABLES							
	W	IM	Y_1	I_1	I_2	Y_2	Y_3	I_3
Father's Occ-NORC (X)	.099	.031*	.078	.001*	.012*	014*	.084	017*
Son's education (U)	.562	.011*	294	118	.238	.126	.122	134
Occ-NORC, Marr. (W)	...	.205	.545	139	105	.068*	045*	−.023*
Income, Marr. (IM)	...	...	−.061	.281	− 050*	023*	− 004*	.054*
Occ-NORC, I (Y_1)	...	...	...	130	.538	047*	.203	.057*
Income, I (I_1)	...	...	...	...	.033*	.492	− 030*	.089
Occ-NORC, II (Y_2)	...	...	...	...	..	.121	437	.002*
Income, II (I_2)	...	...	...	...	...	...	.045*	269
Occ-NORC, III (Y_3)	...	...	...	...	...	.	...	.264
Income, III (I_3)	...	...	...	. .	..	. .	...	. .
Coefficient of determination	.363	.050	.603	230	.649	475	.598	409

* Coefficient less than twice its standard error

the net regression coefficients for the effects of X on occupation and income (although all the partial coefficients for the income-dependent variables are less than twice their standard errors and are assumed to be statistically nonsignificant). Rather, at the third panel, the magnitude of the direct effects of X on son's occupational achievements are only slightly smaller than at the time of marriage.

These "lagged effects" of social origins on son's occupational destination at midcareer are somewhat puzzling since they cannot be accounted for by an increase in occupational inheritance over the course of the restudy period, by any change in the relationship between paternal occupation and son's education or other intervening achievements, or by the effect of some unmeasured background variable such as paternal education, which asserts its influence as some time-lagged family factor associated with life-style goals (Featherman 1969, chap. 6). It is somewhat interesting that a similar nonmonotonic pattern appears in the path coefficients for paternal status in the synthetic and real career models in figures 1 and 2. Assuming these small changes in the equally small coefficients reflect more than changes in measurement error, we can offer no substantive explanation of these "lagged effects" of paternal occupation at midcareer of the son.

The direct effects of education on the socioeconomic career appear to influence successive occupational and economic statuses differentially. In table 2 the β coefficients (partial regression coefficients in standard form) for the effects of U on the four occupational statuses decline monotonically, while those on income statuses rise monotonically. The greatest direct impact of education on occupation for men of equivalent social origins occurs rather early in the work career, and with the accumulation of occupational experience and job tenure this direct influence of education declines. However, the direct role of formal schooling on the amount of salaries and wages earned at these four points in the life cycle becomes more pronounced as older men of similar backgrounds and occupational statuses translate their formal education into occupationally-relevant experience and job tenure. Put another way, regardless of one's social origins, inexperience is an economic handicap for men beginning their careers at the same occupational levels and with the same formal schooling as men who have more experience and tenure.

Beta coefficients for the net effects of W on Y_2 (.105) and of Y_1 on Y_3 (.203) support our earlier remarks about the historical quality of occupational careers, namely that there is a modest degree of continuity in the rank order of occupations achieved over a career which is evidenced by positive nèt relationships between statuses more remote from current job. This observation is true even though the largest direct (net) influence on occupations at Panels II and III (Y_2 and Y_3) is attributable to previous occupational status and even though occupation at marriage bears

no statistically significant net relationship to occupational level at midcareer (Y_3). Unfortunately, the unequal temporal intervals between our panel measurements obviate any discussion of the trend in the size of β coefficients for occupational statuses at adjacent points in the life cycle (i.e., W and Y_1, Y_1 and Y_2, Y_2 and Y_3).

The net impact of occupation on income at the four points in the life cycle is positive, but the coefficients are unstable, declining from marriage (.205) through Panel II (.121) and then rising in magnitude by Panel III (.264). These β coefficients are consistent with an interpretation of the relationship between occupation and income (for men of equivalent social origins, education, and other prior achieved status) which posits that men forgo current income remunerations shortly after entry into the labor force in seeking and attaining those jobs with higher long-range (but lower short-range) earning potentials. Alternatively, but not incompatibly, the geographical and horizontal occupational mobility of some men during the early years of employment may prevent them from accumulating tenure or seniority with a specific company, producing some attenuation in the net relationship between occupation and income in this period. We lack data to test either of these speculations, and they remain as conjectures for further study.

As noted above for the effects of occupational statuses on dependent occupational variables, prior income attainments affect later economic achievements. For example, income at Panel I affects Panel III income net of background, education, occupational level, and Panel II income. Additionally, the most recently antecedent income variables bear the strongest direct effects on each successive economic status. However, there is less stability in the economic structure or in the economic achievements of men than exists in the occupational structure or in men's occupational achievements in the period of the life cycle considered here. To illustrate, all β coefficients in table 2 relating income variables to economic achievements are of a smaller magnitude than the coefficients relating occupational independent variables to occupational achievements. Moreover, the coefficient of multiple determination (R^2) for income achievement is smaller than that for occupational achievement at each point in the life cycle. This lesser stability is partly a function of unadjusted interperiod effects like inflation, but only where the latter is expressed differentially in the PFS sample.

Finally, although our model of socioeconomic achievement in table 2 permits us to explain more about occupational and economic attainments at midcareer than at the time of marriage, the rise in the R^2 values is not monotonic. Specifically, our model explains slightly less about achievement at Panel III than at Panel II. This undoubtedly reflects idiosyncracies of

the Panel III data,[6] rather than any general trend in our target population. In any case, the more elaborated social structural model accounts (at best) for 65 percent of the variance in occupational achievement and for 48 percent of that in economic achievement. While the addition of historical effects of prior on current achievements has improved our representation of the basic structure of the process of socioeconomic achievement up to midcareer, we need to explore this process in greater detail.

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[6] As a result of reinterviewing for Panel III in the years 1963–67, men in the PFS data experienced unequal intervals between Panels II and III. Although adjustments for this result were introduced prior to analysis, and the residual effects on the findings have been minimal (Featherman 1969, chap 4), remaining error from this design characteristic is interpreted as measurement error in the dependent variables Such error would attenuate the R^2 values but leave unaffected the β coefficients for the independent variables (assuming uncorrelated errors)

Commentary and Debate

A CRITICISM OF SCHEVITZ'S REVIEW OF PERRUCCI AND GERSTL

Schevitz's "review" of two books by Perrucci and Gerstl (*AJS* 76 [January 1971]: 774–76) devotes most of its two-plus pages to what is not in these books and gives little attention to what is. What the authors have "neglected," it appears, is Schevitz, his opinions, the political causes he favors, and his attempt to portray the engineering profession selectively in the light thereof.

Quite apart from the puerility of Schevitz's remarks, is it not a responsibility of the editors to reject reviews which violate so flagrantly the canons of book reviewing?

Albert D. Biderman

Bureau of Social Science Research

THE REVIEWER REPLIES

Following Biderman's "canons" of book reviewing, a reviewer of a book that explained population growth as a function of the number of storks would have to avoid discussing anything but the method of counting babies and storks and the statistical relationships discovered between the numbers of babies and storks. As far afield as one could go by following Biderman's guides for book reviewing would be to note that the statistical relationships are probably conditioned by the number of available chimneys. Biderman's criticism implies that the canons of scholarly activity require a reviewer to avoid explaining the phenomenon under examination if the book's author did not include the variables that would explain the phenomenon.

Contrary to what Biderman charges, it is Perrucci and Gerstl who "portray the engineering profession selectively," leaving out the most significant variables that explain important aspects of the social organization of engineering and its relationships to other parts of American society. When one book's title purports to explain the relations between an occupational group and the social system as a whole and the other book's title the organization of the engineering profession, it certainly seems appropriate for a reviewer to point out significant variables the authors neglected.

Biderman's distress obviously stems primarily from the role I ascribe to the Cold War and an expanding American imperialism as the most significant determinants of the present shape of the engineering profession

in terms of its size, mix of specialties, predominant type of work organization, the locations and flow of engineering manpower, and fluctuations of engineering unemployment. Although there is an abundant literature on science policy that supports my hypotheses, many of the sociological studies of scientists and engineers, like Biderman's own research, are funded by the air force, which would not accept *capitalism* or *imperialism* as variables. For the air force, capitalism and imperialism are *givens*. The air force's studies explain variations in recruitment, motivation, career patterns, etc., within the given framework of corporate capitalism and American imperialism.

Although it is legitimate for a scientist to control some variables in order to understand the effects of other, more significant variables, the choice of control variables by Biderman, the air force, and Perrucci and Gerstl is constrained by their commitments to American capitalism. The result of such commitments is to avoid examining the variables that have had the most significant effects upon science and engineering since World War II.

I am heartened by the editor's rejection of Biderman's suggestion of censorship of views and approaches that violate established "canons" or norms. If Biderman's attempts at suppression of ideas reflect the increasing militarization of our society, the editor's rejection of such attempts reflects some healthy countertrends.

JEFFREY M. SCHEVITZ

Washington University

A CRITICISM OF COLFAX'S REVIEW OF PARSONS

I was rather unhappy to read the recent review of Talcott Parsons's *Politics and Social Structure* (1969) by J. David Colfax (*AJS* 76 [March 1971]: 932–36). As members of the profession know, Colfax belongs to its radical wing and would find anything Parsons writes distasteful. On the other hand, it can also be said that an admirer of Parsons (like me) would be predisposed to write favorably about almost anything Parsons chooses to discuss. Perhaps sociology is so polarized that no one can be found in the middle ground on the subject of Parsons, but I do not think this is true. Surely someone could have been selected to assess the strengths and weaknesses of a publication by one of the great theorists of our time.

Sociologists unfamiliar with the individual articles in the collection might expect to find in the *Journal* some inkling of what Parsons is driving at and not mere negativistic ranting. But ranting is what the reader gets

from Colfax—punctuated by quotations torn out of context so as to better mislead the unwary. Thus Colfax quotes as follows from Parsons's article on the (Joseph) McCarthy era:

> The changed situation in which we are placed demands a far reaching change in the structure of our society. . . It demands above all three things: The first is a revision of our conception of citizenship to encourage the ordinary man to accept greater responsibility. The second is the development of the necessary implementing machinery The third is national political leadership, not only in the sense of individual candidates for office or appointment, but in the sense of *social strata where a traditional political responsibility is ingrained* [Colfax's italics]
>
> The most important of these requirements is the third Under American conditions a politically leading stratum must be made up of a combination of business and nonbusiness elements The role of the economy in American society and of the business element in it is such that political leadership without prominent business participation is doomed to ineffectiveness and to *the perpetuation of dangerous internal conflict* [Colfax's italics] . . Broadly, I think, a political elite in the two main aspects of "politicians" whose specialties consist in the management of public opinion, and of "administrators" in both civil and military services, must be greatly strengthened *And along with such a specifically political elite there must also be close alliance with other, predominantly "cultural" elements, notably perhaps in the universities* [Colfax's italics] [Pp 177–78]

Compare the second paragraph as Colfax adapted it with the paragraph written by Parsons:

> The most important of these requirements is the third Under American conditions, a politically leading stratum must be made up of a combination of business and nonbusiness elements The role of the economy in American society and of the business element in it is such that political leadership without prominent business participation is doomed to ineffectiveness and to the perpetuation of dangerous internal conflict. It is not possible to lead the American people *against* the leaders of the business world. But at the same time, so varied now are the national elements which make a legitimate claim to be represented, the business element cannot monopolize or dominate political leadership and responsibility. Broadly, I think, a political elite in the two main aspects of "politicians' whose specialties consist in the management of public opinion, and of "administrators" in both civil and military services, must be greatly strengthened. It is here that the practical consequences of McCarthyism run most directly counter to the realistic needs of the time But along with such a specifically political elite there must also be close alliance with other, predominantly "cultural" elements, notably perhaps in the universities, but also in the churches

Note, first, that the two deleted sentences contain an explicit denial of the likelihood of business domination of political leadership in the United States, not approval of it. Second, in the concluding sentence, which Colfax indicated he italicized, he also made two unindicated changes.

He changed the beginning "But" to an "And," and he left out the concluding reference to the churches. These editorial changes contribute to an image of Parsons as a political bedfellow of Spiro Agnew. I would not like a student to resort to these tricks on a term paper.

To conclude: the theories of Talcott Parsons have been developed during a lifetime of intellectual effort. In my opinion, they deserve the respectful attention of the profession because they help to explain sociological reality. True, they are couched in abstract terms, so that their relevance to the real world is not always obvious. But sometimes they address contemporary problems so clearly that those who disagree with his conclusions know that he and they cannot both be correct. For instance, one of the articles described by Colfax as "one of the most wrongheaded essays on race written in the sixties" shows Parsons assessing the prospects for racial reconciliation in the United States. On the basis of his theories of social and cultural integration as well as his observations of American society, Parsons was optimistic about the possibility of full inclusion of Negro Americans in the mainstream of American society *without a loss of identity,* that is, without destroying the solidarity of the Negro community. Parson's optimism is not shared by many sociologists, including specialists in race relations; whether he is correct remains an empirical question. Still, Parson's optimistic assessment of American race relations published in 1965 seems more nearly correct in 1971 than Lewis Killian's pessimistic assessment published in 1968. The United States is a long way from full inclusion but has moved in that direction, not toward the *apartheid* model of South Africa. If Parsons does not prove "wrongheaded" on this issue, this will not validate his theoretical position, but it ought to make radical sociologists less certain about their apocalyptic vision of Western society.

JACKSON TOBY

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COLFAX REPLIES

Perhaps it is because Jackson Toby is so intent upon labeling my appropriately negative review of Parsons's *Politics and Social Structure* as consisting of "ranting" and "tricks" that he shows himself so pathetically incapable of providing a single compelling reason why that work should have been cast in anything other than an unfavorable light. After providing a bit of pop sociology-of-knowledge ("Perhaps sociology is so polarized . . ."), Toby accuses me of using "quotations torn out of context so as to better mislead the unwary." Insofar as there are sociologists who are "unwary" of the political content and implications of Parsons's writings,

they are so only by choice, and as such their protection is not my concern—nor should it be Toby's. There are no "innocent" professionals today—only the functionally ignorant.

To the rather sophomoric charge that I have "torn" quotes from context, I would only note that any quotation is literally "torn out of context"; here, the only question is whether the context provides a meaning different from the one represented. In a rather short review I quoted quite liberally from Parsons's writings in an effort to avoid the—not unexpected —charge that I was misrepresenting his ideas. The careful reader who compares Toby's and my extracts from Parsons's article on McCarthyism will see that they say essentially the same thing: Parsons's values and political preferences are perfectly clear in both the full and the slightly truncated versions. Toby either does not understand or does not care to understand what Parsons is saying, and in his search for some evidence of heresy he has mistakenly seized upon ellipses as the mark of the sin of omission.

To the charge that I have not been sufficiently "respectful" of Parsons's "lifetime of work" I plead guilty. Deference has no place in critical discourse, and my difference with Toby on this point most likely reflects our different biographies and needs. But that an admirer of Parsons such as Toby sees fit to invoke such "particularistic" criteria in this context is highly revealing.

Finally, Toby's concluding remarks are simply execrable. In a garbled paragraph in which he reaffirms his faith in Parsons's "optimistic assessment of American race relations," Toby notes that whether Parsons is correct or not "remains an empirical question." It *is* an empirical question, and Toby's ability to maintain his illusions in the face of the evidence—continuing black rebellions (e.g., June 1971), rising unemployment among blacks, the continued deterioration of the (black) inner city, and official repression of black activists—is nothing less than astounding. Toby, in asserting that "still, Parsons's optimistic assessment . . . seems more nearly correct in 1971 than Lewis Killian's pessimistic assessment published in 1968" suggests either that he is remarkably ignorant or that he gets his data from Mr. Moynihan. There *is* a difference—which Toby seems incapable of apprehending—between "apocalyptic visions" and sociological realism. In any event, I would be the last to attribute Toby's views on these matters to Parsons; I doubt that he would want the credit.

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A BRIEF COMMENT ON TIRYAKIAN'S REVIEW OF CLARK

I sincerely hope that the error made in referring to Parsons's *The Structure of Social Action* as *The Social Structure of Action* in the first line of Professor Edward A. Tiryakian's review (*AJS* 76 [May 1971]: 1146–48) was not a typographical error on the part of *AJS*. I also sincerely hope (and this is really reaching!) that it was by design that Tiryakian reworded the title of the Parsons volume. If the error was typographical my newfound revelation (about to be revealed) becomes only a result of the "mysteries of the universe" (what we call "chance" in sociology). *But,* if the error comes from the pen of Tiryakian, whether by design or "slip" (preferably, of course, by design), the whole spectrum of human behavior and motivation raises its enigmatic head and becomes fair game for comment by those of us still wondering what "this thing" around us is all about. And now, the revelation.

Tiryakian's "error" clarified almost ten years of concern over what it was that was *wrong*. with Parsons's argument in *The Structure of Social Action*. What was (is) wrong with the argument of Parsons is indicated in Tiryakian's "error": the structure underlying social action, for Parsons, is *not* "social" at all (as exemplified, for example, in Parsons's discussion of "conditions" vis-à-vis action in *SSA*). Parsons was right in calling his work what he did—Tiryakian's title is, at best, wishful thinking. Possibilities raised by the "error" in Tiryakian's review, however, are fascinating. Consider: *The Coming Sociology of the Western Crisis,* or "Existential Sociology and the Phenomenological Tradition," or *The Religious Forms of the Elementary Life*, or . . .

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RIPOSTE TO GOODWIN

Glenn Goodwin's astute comment on my review of the book edited by Professor Terry N. Clark is not only thought-provoking but also has led me to soul searching about the calamitous cause of my *lapsus calami*. Yes, *mea culpa,* it was not a typographical error on the part of the *AJS*. After initial agonizing at the horror of an inverted title, I have come to the conclusion that my "error" was not the result of conscious design nor was it a simple slip. The slip, quite likely, may have been the vehicle of the Spirit which moves in mysterious ways, as it always does in propelling us to new truths and insights. And, considering as I do that Talcott Parsons's *The Structure of Social Action* is one of the few seminal works in sociological theory written in modern times, is it any wonder that the

Spirit moved me to inverting the title? For a seminal work is one that contains layers of meaning which are not always discernible upon first reading it, and thus "The Social Structure of Action" may be a value-added dimension of *The Structure of Social Action.*

Goodwin's suggestions in his last sentence are indeed fascinating. What great wines are yet to be brought out from old sociological casks! And all this stemming from a modest review of *On the Influence of Social Communication*—oops, there goes the Spirit again!

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Review Symposium

The Coming Crisis of Western Sociology. By ALVIN W. GOULDNER. New York: Basic Books, 1970. Pp. xv+528. $12.50.

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Social theories are also social doctrines which contain hidden tenets about human nature and social reality. Unmasking them is a critical aspect of social analysis, because it alerts us to the actual social consequences of a theory and the interests it serves—and reveals the questions a theory cannot raise or dares not pose. Such unmasking neither invalidates a theory nor even necessarily lessens its cognitive utility; on the contrary, by becoming aware of the extratheoretical (or unconscious) meaning in a theory, we are more likely to be able to transcend it in thought and action.

In Alvin W. Gouldner's richly suggestive and provocative study, he elaborates, specifies, and applies this generic theorem of the sociology of knowledge in the course of his excellent critical interpretation of the rise of functionalism and his devastating "critique of what is by far," in Gouldner's view, "the dominant system of social theory, namely, that created by Talcott Parsons" (p. 14). Unfortunately, Gouldner's significant contribution is flawed by a tendency toward theoretical hyperbole: often he appears to abandon the standpoint of critical rationality and science, as in abundant assertions like the following: "*Objectivity* is the way one comes to terms and makes peace with a world one does not like but will not oppose; . . . *objectivity* is *the ideology* of those who are alienated and politically homeless . . . [and] of those who reject both the conventional and the alternative mappings of the social order" (p. 103, my emphasis). These assertions are woven into the texture of Gouldner's critique, which fails to distinguish between or grapple with the problem of how *detachment* or *commitment*, on the one hand, and *objectivity*, on the other, are reciprocally interrelated. He verges on sociological solipsism, so that "ideology" seems to swallow up and absorb all valid knowledge of the objective world. He seems to forget that our understanding of the objective world is socially determined, not merely our misunderstanding. Social theories all but become transformed, in the course of Gouldner's discussion, from imperfect and limited answers to specific questions—from summary explanations which others can understand and test by practical activity, into mere "mappings" of the social world in competition with other such equally subjective "mappings." This results, I think, from Gouldner's preoccupation with his detailed critique of Parsons's neoscholastic conceptual scheme—a *non*-theory having few empirical references, let alone testable propositions about social reality. Because Parsons's proliferating conceptual scheme was elaborated without connection to a set of substantive problems, Gouldner himself tends to become absorbed in a similar exercise, a sort of inverted biblical exegesis of the "errors" of Professor Parsons. In

consequence, Gouldner neglects the central problem of how we do, in fact, separate the *ideological* from the *truth* content in our theories of the world.

Gouldner tells us, properly, that "the old society maintains itself through theories and ideologies that establish its hegemony over the minds of men," and that emancipation from the old society requires a "critique of the social theories dominant today" (p. 5). However, in his effort to avoid "a pseudo-systematic and exhaustive coverage," he neglects highly "important standpoints" embedded in empirical social research in sociology, and not merely in what is self-defined as "theory." To disclose Parsons's metaphysics (and even Goffman's dramaturgy) is valuable; but given his wish to "criticize" theories which he believes maintain the ruling order's ideological hegemony, his neglect of the *engagé* liberal social research which dominates American sociology is a basic failing.

The presentation of propositions about social reality as findings of empirical social research—even in antiseptic language and a studied style of detachment, or in the form of multivariate quantitative tables—scarcely alters their "theoretical" status, nor does it eliminate the role they play, when suitably interpreted, in confirming or disconfirming a given world view. By what these theories (or propositions) ask and how it is asked, by the relationships examined and the interconnections sought, social research takes on irreducible political meaning: such meaning—the aspects of the social order from which given types of research and theory deflect attention, and the thoughts and practice of human beings which they stifle or paralyze—can be revealed by critical analysis of the ideological content of research and theory. Such a critique, however, is inherently limited—both as polemic and valid knowledge—unless it is combined with reasoned empirical investigations of the world which start from different premises, ask different questions (or the same questions differently), seek relationships not sought in other theories, and counterpose explanations of substantive problems which make more sense and which can be tested and modified by our practical activity.

By focusing on Parsons to such an inordinate extent, Gouldner fails to analyze the incredibly skewed body of work on "social structure" which—well beyond the reach of the Parsonian "network"—focused on this or that trivial or peripheral aspect of it, but ignored the historically specific structure of ownership and control of property and its ramifying imperatives. Scholasticism in social "theory" has coexisted congenially not only with an abstracted empiricism (Mills's term), but a special type of liberal ideology which in the past several decades crept into the pores of virtually all social research. Gouldner certainly knows this, but in his book he scarcely attends to it as a substantive problem, despite his advice to "radical sociologists" that "a critique of sociology . . . requires detailed and specific analyses of the dominant theoretical and intellectual products that sociology has created" (p. 14). (There are several excellent pages of discursive discussion of the "liberal technologues" [pp. 500 ff.], and of the welfare state's pressures on social science [pp. 341 ff.]. But even here, Gouldner is absorbed by Parsons.)

Moreover, by confining himself to sociological theory, Gouldner fails to emphasize that not only in sociology (and I believe, not even primarily), but in all academic social science there is an underlying conservative (liberal) metaphysic; it is assumed that the social order in the United States (and capitalism in general), albeit imperfect, is essentially just, harmonious, and timeless, rather than historically changing, temporal, and as likely as other social orders to be transformed by emerging social forces and the activities of rational and creative human beings. Between "the dominant theoretical and intellectual products" of academic sociologists and those of academic historians, economists, anthropologists, and political scientists, there has been little difference in ideological content. Each has reinforced and fed the other.

In the aftermath of World War II, historians suddenly discovered that the formerly denigrated Robber Barons were, instead, bold innovators; they "found" that "populism" and "status anxiety" were at the roots of McCarthyism, but they neglected its ideological sources in the Cold War and its relationship to the Truman Doctrine and Dulles's policies, and its propagation and backing by men of power. Among economists, a "new capitalism" emerged without capitalists; large corporations appeared which were guided not by profit considerations but "satisficing" maxims, and were said to be possessed, if not of a "soul," at least of a "conscience." Political scientists and sociologists marveled at the virtues of pluralism and the effectiveness of veto groups, but ignored the concentration of power and the interpenetration of political and economic relations at the national level—and concluded, incidentally, that a certain degree of political apathy in the masses was "healthy" if not necessary to this wondrous form of political democracy. Liberal sociologists concluded that mass movements were inherently antidemocratic; the workers were found to be both authoritarian in psychology and childlike in understanding, and not to be trusted with power, unless organized and led by properly responsible men. Poverty was found to exist in "pockets" or to be the result of some quality peculiar to the family or individual involved; miraculously, no connection was either sought or found between such suffering and the capitalist structure of ownership and control. "Underdevelopment," far from having anything to do with colonial subjugation or imperialism, was said to be the result of an absence in such nations of the proper rational and moral faculties, or achievement orientations. Their indolence was the cause of their misery. Finally, all around, the social sciences were "discovering" that the major problems that had hitherto characterized advanced capitalist societies, were solved in the United States and that socialism had some symbolic value for the backward but was no more than a chimera in "modernized" societies; conveniently, there was now an "end to ideology," as the differences between left and right disappeared with the triumph of the "democratic revolution in the West."

These interrelated propositions and "findings" of the academic social sciences are all but ignored by Gouldner, despite their obvious impregnation by American corporate liberalism. (In his insightful discussion of

Parsons's views about some of these issues [pp. 286 ff.], Gouldner acts as if Parsons were their progenitor and sole proprietor, whereas, of course, Parsons was accepting from others—sociologists and nonsociologists—just such "findings" as appeared congenial to his view of the world, and which could be enclosed in his conceptual boxes.)

In part, Gouldner's lack of attention to the interrelationship between dominant modes of thought in the other social sciences and in sociology may reflect his own unconscious acceptance of the view that there can and should be such a thing as a "general theory" distinctive to sociology. Notwithstanding his occasional remarks which seem to indicate the contrary, and his very fine discussion of property relations as the "infra-structures" of social systems (pp. 304 ff.), this is the view which the book as a whole seems most closely (in Gouldner's favorite word) to "resonate." It is a view that I reject, and I believe that sociologists must reject if they are to do their best work. A substantive social theory that can be contained by departmental boundaries is worth little, except for doctrinal purposes. The academic division of labor along bureaucratically determined lines between "disciplines" that, on the one hand, separates the major aspects of social reality from one another and, on the other, excludes certain questions from systematic consideration altogether, is both cause and consequence of the limitations of contemporary academic social science

Despite his exhortations (with which I agree) to the New Left not to neglect self-conscious theory, nor to succumb to antiintellectualism or to a "hastily-gulped, vulgar Marxism" (pp. 5–6), Gouldner ignores the emerging corpus of writing by young radical and Marxian scholars that refutes the dominant and received intellectual products and theories, and cuts across the present "disciplines." Bound also, it would seem, by the very "professionalism" he rejects in his declaration of the need for a "reflexive sociology," Gouldner also ignores the nonacademic-centered social research and theoretical activity of people in the "Movement" appearing in New Left journals, newspapers, and pamphlets which attempt to show (some quite successfully, and certainly more so than many orthodox professors) how this country is ruled, by whom, and with what consequences here and abroad.

If there is ferment in the social sciences today, as Gouldner agrees, it is the result, to a considerable extent, of the theoretical and practical activity of the New Left within the university and in the society as a whole, where it has launched assaults against the prevailing order. Thus, it is remarkable that this development should receive such scant attention in a work whose stated objective is to contribute to our understanding of how "theory-products and theory performances are generated and received" (p. 483). Similarly, while Gouldner tells us that academic "social theorists" have "given little systematic analysis to the role of the university in shaping social theory" (p. 403), his own study also fails to examine systematically how the general milieu in academia, where most sociologists have worked over the years, has affected the development of sociological "theories." In particular, he does not attend to the impact of the integration of the

university into the political economy of war; neither the post-World War II dominance of Cold War scholarship nor the more recent rise of counter-insurgency studies receive serious attention—none, certainly, commensurate with its importance or the care given to Parsons's wondrous world of make-believe. Nor is serious attention given to the implications of the fact that the research of many prestigious professors at the centers, institutes, and interdisciplinary programs of leading universities has been funded, and its objectives have been coincident with, if not determined by, the CIA, the Air Force, or Army research offices. (At least thirteen programs at Harvard between 1960 and 1966 received "no strings attached" funds from CIA conduits in the known amount of $456,000; recipients of such funds for their personal research included some of Parsons's colleagues in the Department of Social Relations.)

In his preoccupation with Parsons' personal experiences, Gouldner also fails to examine at all how the post-World War II wave of political trials under the Smith Act, the rash of congressional investigations, the black lists in the media, the trial and execution of the Rosenbergs, the attacks on left-wing intellectuals in and out of the university, the loyalty oaths, and the justification of such purges by leading academic lights like Sidney Hook—as a special type of Cold War liberalism fastened itself upon the country—affected the entire climate of social thought in which neoscholastic, abstracted empiricist, and apologist social research co-prospered. Certainly it was not because of the conceptual prestidigitation and evangelical enthusiasm of Parsons and his early "seed group" of students who became prominent professors of sociology, that an entire discipline, with few honorable exceptions, abandoned the focal concerns of the classical social theorists. The post-World War II deflection of sociology, and social science in general, from consideration of "controversial" questions into allegedly "value-free" studies had social determinants rather deeper than this. (*The Social System* was published in 1951, at the height of McCarthyism. It certainly could be read and its theory embraced without fear of being considered "subversive." How this general social situation affected the ascendance of Grand Theory is a question conspicuous by its absence in Gouldner's study. This is in marked contrast to his very provocative attempt to link the genesis of Grand Theory to the crisis of the 1930s as a sort of "alternative to Marxism" that could safely attract "young intellectuals who were under pressure to respond to Marxism" [p. 177].)

That these same "young intellectuals" now several decades older, including Parsons himself, should, according to Gouldner, now be "moving toward a convergence with Marxism" is quite startling indeed, if true. I consider the "evidence" for this "convergence" presented by Gouldner superficial, and the thesis incorrect (pp. 354 ff., 368 ff.) An eclectic utilization of certain Marxian concepts shorn of the general theoretical framework, or even theoretical "orientation," characterizes the work of several well-known anti-Marxian sociologists. Indeed, as Lewis Feuer has pointed out, "most contemporary political sociology consists of glosses to Marx." However, this is not because of any so-called theoretical convergence, but

because several of these sociologists were themselves Marxists when younger and continue to be influenced by Marxian ideas, while devoting their work to the refutation of these ideas. However, Gouldner is not even referring to the work of such men, but rather to others like Neil Smelser, Wilbert Moore, and, lo and behold, even Talcott Parsons himself. Gouldner argues from superficial similarities between certain elements in Marx's and Parsons's writing, abstracted from their theoretical context. (Among them are "voluntarism" in the young Marx and in Parsons's scheme of things. On this basis, there is a "convergence" between Parsons and Lenin also!) Gouldner also notes that even Parsons recognized the need to attack the class base of the Junkers to prevent a resurgence of Nazism in Germany. Can this mean anything but that on those rare occasions when Parsons addresses a substantive theoretical problem, he is forced either to abandon his own conceptual scheme or all sense of historical reality? Similarly, because Moore belatedly discovers that "Marx took fairly full account of the purposive character of social action," or that Smelser finds Marxian concepts useful to overcoming "limitations" in functionalism, scarcely means that there is a theoretical convergence. It means only that some functionalists have finally decided to recognize the existence of a powerful competing theory that is attracting younger social scientists, rather than to ignore it or discount it as they did in the past. Such diplomatic recognition of a hostile theory in a time of "crisis" is merely good statesmanship and is not to be confused with substantive theoretical convergence. A social theory which fails, as functionalism does, to tackle substantive problems and relate them to the historically specific form in which political economic imperatives ramify throughout the social structure can contribute little to our understanding, or to our ability to act in and change the contemporary world. Only a social theory which compels us to stand outside our own society and to see it as a historical whole, which questions the society itself rather than accepting it as given, which focuses on the causes of its development, its inner tendencies, and their ramifying consequences, and on the forces within the society that may lead to its transformation—only such a theory may allow us to go to the root of things and lay them bare.

* * *

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The crisis of academic sociology is evident everywhere and aggravated by the fact that sociology is at one and the same time the actor and the object of its critique. Gouldner's book is first of all an expression of this crisis. Its publication gives evidence of the distance traveled since the presidential discourse of Kingsley Davis or E. Shils on "The Calling of Sociology." Sociology can no longer be identified with functionalism; it is no longer the discourse of society about herself. It is natural that this

critique of academic sociology speaks first about what social control conceals or represses. Gouldner calls this recognition of the social and personal conditions of sociological work "reflexive sociology," opposing a pretension of objectivity which had never been acceptable to many foreign observers, who have never ceased to see in most of American sociology a product of the American society, as linked to its time and place as a Balzac novel was to its place and time.

For several years, in the United States, as elsewhere, this dispute has been rather widespread, making it useless to detail here. But it is impossible for a sociologist to be satisfied with such critiques.

If sociology is only ideology, the sociologist who critiques sociology is doing ideology, and sociological research is reduced to a debate of opinions. Such is the position of certain radicals who reject even the existence of sociology as a valid science, and consider it to be merely an instrument in the service of a ruling class.

How far along these lines does Gouldner want to go? It is difficult to know, since he does not pose the central problem: how to see through the ideologies to the formation of positive knowledge. Or, moreover, how to recognize the relations that at a given time unite the social and cultural limitations of a field of knowledge and the scientific character of this knowledge. Gouldner opposes academic sociology with an attitude rather than with analyses and hypotheses. Certainly, the recognition of the personal and social determinants of sociological thought allow him not to see himself naïvely for "the bird of Minerva." But, from here on, does one want to return to taste and opinion? If not, what can we propose to avoid overextending the poor powers of *Verstehen?*

It seems to me Gouldner places in opposition two eternal schools of thought: the school of social order from Plato to Parsons and the school of change which is perhaps that of Marx. But the relations between these schools remain altogether obscure; as presented by Gouldner, Western sociology is formed of two currents which separated after Saint-Simon. In effect, Marx and Durkheim derived equally from Saint-Simon. But Gouldner sees today the rapproachement of these two schools. What would that mean? One can speak of the common conservatism expressed by the socialist ideologues as much as by the capitalist or one can show a reciprocal opening in these two schools, or one can, finally, combine a purified Marxism with structural functionalism into a structuralist Marxism.

Gouldner's critique is limited because he did not ask himself about sociological knowledge and about the history of this science or nonscience. This is all the more so because he is too vague at the level of the ideological critique itself. By opposing the romantic youth to the middle-aged university bourgeois, he avoids the most radical and violent attacks which he does not bring up and which he does not answer in this call to individual creativity.

We have to choose: either refuse to recognize the existence of sociology as positive knowledge and denounce it as a ruling class ideology, as the mask of bourgeois society; or recognize its existence and face head-on the

problem of relations between science and ideology in sociological knowledge, in functionalism as in other intellectual tendencies

I place myself in this second perspective, because as I see the first defended today, it does not seem to provide a realistic method of analysis.

I will make a central hypothesis the current crisis of sociology is really the birth of sociology. The history of sociology shows, through conflicts and oppositions, the growing possibility of a sociological analysis

At the beginning of the nineteenth century, sociology seemed to be the ideology of a rising ruling class, far from holding political power or even a strong economic position The positivism of Saint-Simon and Comte is the expression of the new society born in the French Revolution and the English industrial revolution

Historical evolution and cultural values were identified Idealism and materialism were united in this positivism that believed in ideas and in science, and that ideologically assured solution to class conflicts which were not organized yet. Science was associated with love, body with soul, elite with mass, man with woman

Historicism, or philosophy of history, made positive knowledge impossible, since any given piece of information could be validated only in an interpretation of the whole evolution This introduced, however, as Gouldner points out, the theme of the social system as a system of social relations and activities, and not as the manifestation of principles or virtues, as in the "Enlightenment "

This unity was shattered by industrialization and the appearance of class conflict. Utilitarianism transformed itself into social thought with many ramifications, whereas Marxism, on the other hand, was founded on an analysis of social contradictions and described the institutions and the values as the ideological mask of domination

Industrial development and pressure by labor exerted through unions and the state brought about the reappearance of the autonomy of political institutions. Therefore Durkheim posed the problem of social integration in a society ever more differentiated and rapidly changing. He keeps the historicism which is the model of knowledge for the nineteenth century, but he also begins to destroy it as he raises his ideas about the social system.

At the same time, Weber bypasses historicism on its other flank by comparative analysis and his construction of ideal types

The thrust of this very short resume is to underline two essential points In the first place, we know that the nineteenth century was dominated by the accumulation of capital and thus development by factors that can be analyzed in economic terms, such as capital and work Social thought used historicism as the only possible way of understanding the relations between economic system and social change In its *épisteme*, to use the word of Michel Foucault, the geometrician God of the classic age is replaced by the history of humanity; but social facts are not yet analyzed as elements of a system. In the second place, within this historicism there is a succession of three phases which correspond to the evolution of class relations:

the initial phase corresponds to the utopia of a rising class; then an explosion corresponding to the brutal confrontation of the classes; and finally the appearance of the notion of social system which corresponds to the beginnings of the institutionalization of this class conflict.

Sociology as it has developed, especially since World War II, belongs to another *épisteme*, where it can be positive knowledge; although we again find three lines of thought: the initial utopian phase, the recognition of contradictions, and finally the analysis of social integration. We find, then, a diversity of schools comparable to those described for the nineteenth century, but in a new cultural field, where sociology becomes possible.

Sociology can appear only when social organization and social change are recognized to be two faces of the same reality; that is to say, when economic growth is analyzed in terms of social organization and not only in terms of economic accumulation. The roles of science, technology, and education; of planning and decision making; of communication systems; and of organizational structure as determinants of economic growth and social change become central. This type of study does not dispense with analysis in terms of social classes in the European sense of the term; it simply necessitates the development of a true sociological analysis of class. This now defines a situation where social facts can only be explained by systems of social relations, where the dichotomy of values and economic contradictions can be bypassed, where all recourse to the idea of man, to the sense of historical evolution—the last strongholds of the old idealism—can be eliminated.

The concept of social system becomes central. Functionalist thought is based on the work of Durkheim and Weber, but Parsons made decisive progress by eliminating the exteriority of social constraint, by considering society as a system of action and in introducing a general and systematic analysis begun by Weber only at the end of his life.

In present society this Parsonian functionalism occupies the same place as the positivism of Comte in the society of early capitalist industrialization, and Gouldner's remarks on the rapprochement of these two authors are well taken (see pp. 205, 457).

Order and change are here still associated, and if the notion of social system has become central, it accompanies a new evolutionism that shows through the pattern—variables and the notions of differentiation and secularization. Society is defined by its functioning and by the historical march toward modernity; of course America is seen as the most fully achieved example of this This sociology cannot be the expression of bourgeois capitalism. It is the expression of a technocratic society, and its success in the Soviet Union, long held back by the power exercised in the name of a party of working-class origin, is affirmed by the takeover of a new ruling class, limited only by the ideological control exercised by the power apparatus.

Functionalist sociology is the utopia of new ruling classes, when a new cultural orientation and a new economic system make possible the forma-

tion of a sociological analysis in the same sense in which economic analysis became positive in the first phase of industrialization.

The initial utopia tends, as in the nineteenth century, to split into divergent currents, when social conflicts reappear. On one hand appears a neoutilitarianism as in the work of George Homans. On the other hand, we see a denunciation of social contradictions in the total social and cultural ensemble—not only with production—which sees in society and culture an ensemble of myths, an alienation of human needs, and a system of social control which reinforces and hides the interests of the ruling class. This neo-Marxist critique has been more actively developed in Europe and in Latin America than in the United States, where a great number of currents of thought appear marking the decomposition of utopian functionalism. Some go toward a more sceptical critique. Riesman and Goffman split from Parsons as Stendhal and Flaubert did from Balzac. The dominant society is emptied of its presuppositions of values and norms, acceptability succeeds morality, as Julien Sorel did Rastignac.

Finally, especially in the United States, the third aspect of sociological reflection leads to a vast development of what one might call neo-Durkheimian thought; that is, research on new forms of social integration, but integration into a system based on change rather than into a body of rules. The theories of organizations and decision making, and those centering on transactions and bargaining are examples of this tendency.

Sociology today, therefore, is defined by a system of positions. This system is homologous to the one we can construct for the nineteenth century. But the reproduction of the relations among the positions must not lead us to forget the fundamental transformation of the field of knowledge.

The unity of sociology is not realized, but it is possible. The existence of sociology is a recognized fact, and does not allow superficial explanation.

What should one expect a critique of sociological thought, and of functionalism in particular, to consist of? An interpretative critique that searches for the motivations of the actors, the social circumstances which have influenced their work, is always fragile and always risks closing in on itself in a purely ideological discussion.

The critique must examine the conditions of existence of an integrated sociological analysis. Are the orientations of functionalism compatible with the treatment of problems posed by these other orientations? Such an investigation would demand more space. But we must at least say that there is in Parsonianism both the underlying elements of the old idealism and a constant mixture of cybernetic and voluntaristic tendencies. The society of Parsons has a soul and a will; it is a home and a community; it manages its activities and conflicts. The cultural values and social institutions are seen in rather the same light as the cross and the sword in medieval society.

The Marxist or neo-Marxist critique can only attack the thought which identifies social control with social structure. Functionalism understands reasonably well crisis, anomie, and possibly change. But other approaches remind us that the state of institutions and social relations is the result

(far less stable and self-sustaining than is imagined) of relations of domination and power. There is no place in this functionalist vision of the world for imperialism and segregation, for social conflicts (of which the functionalist interpretation à la Coser is very limited) or for the diversity of processes of economic development.

When Gouldner begins his analysis with what is most explicit and basic for integration, the values and norms, there is no space for conflict, only for deviance and marginality, no space for classes, but only for stratification; no space for power, but only for influence, order, and modernization

Only if one begins by defining a society by its capacity to constitute its experience by its work and its knowledge, and if one, inspired by Marxism, investigates the class conflicts that define the battle for control of this cultural and social field and of social change, only then can one define the characteristics of social relations and social control in terms of functionalist analysis.

I see a possible combination of analyses in terms of class, decisions, and function only by adopting such a perspective, the farthest possible from functionalism, because it does not begin with social control nor with principles, but with work, not with what is conscious, but with what is least conscious.

As far away as one might be from functionalism, one must not forget what it has introduced. From Durkheim to Parsons, along with the transformation of industrial societies, it has made the concept of the social system central; it has taught us not to consider social facts as different from economic facts. It has then eliminated false problems, such as that of causal action among various categories of social facts.

But sociology cannot constitute itself and combine its diverse tendencies except by ceasing to identify itself with the social order, with the status quo, as it stands codified in its laws, its forms of authority, and its social roles. The first condition for the existence of sociological analysis is the refusal to accept the society in the image it presents of itself The notions of values, norms, and roles, far from being the primordial beginnings of society, are but limited, uncertain, and partial expressions of a social order which is at one and the same time the ideology and the instrument of power and the result of limited and provisory transactions

Certainly Gouldner is right in pointing out the split with functionalism. The industrial societies, after their illusions of the Pax Americana and Sovietica, have rediscovered their political and social conflicts. No one would dare today to speak of development only in terms of modernization, of conflicts only in terms of resistance to change, of social movements only in terms of crisis and anomie. But sociology resists this change of perspective, because it is to some extent part of the system of management of society. It is often called upon to give proof of "realism," that is to say, to consider that money and power are only resources—instruments of action which circulate in a society whose field and orientations are defined independently of power and social class. This tendency, if not stronger, is

at least more strongly encouraged in societies of the Soviet sphere, but it is present in other forms everywhere that functionalism prospers.

This school has well expressed the social changes that have rendered sociology possible. Nevertheless, sociology will only constitute itself and bypass the lack of communication between its schools when it has broken with functionalism—when it no longer confounds social relations with social order, social action with social control, and the production of society itself with its functioning.

Gouldner's book contributes to this indispensible critique of the functionalist utopia.

Book Reviews

White Southerners. By Lewis M. Killian. New York: Random House, 1970. Pp. vii+171. $2.95 (paper).

Rudolf Heberle

Louisiana State University

That white southerners should be dealt with in a series on ethnic groups may cause some raising of eyebrows, although the idea is not new and this book is not the first publication to take this approach. Killian, himself a southerner and a sociologist who knows his region of birth and its people in all their complexity, presents us with a thorough and in many respects original study.

Killian defines as white southerner a white person born and raised in the South and who thinks of himself as a southerner, even if, like the author, he has left the South, or a white person who, though not born and raised in the South, lives in the South and identifies himself as a southerner (as, for example, this reviewer). The South is defined as the eleven states of the Old South. Killian calls it "a loose definition" (p. 11), since it excludes many people who would be considered southerners or who think of themselves as southerners, that is, mainly people from the Southwest and from the border states. This geographic limitation, which follows the example set by Howard Odum, is widely accepted and does not make the definition any "looser" than a more comprehensive regional definition would have done. The "looseness" is due to the emphasis upon consciousness. This, however, is good sociology, since it permits the inclusion of (*a*) acculturated migrants to the South and (*b*) the large groups of migrants from the South living in other parts of the country. Why, then, are white southerners a minority or, as Killian says in some instances, a quasi minority? To answer this question, Killian refers to Louis Wirth's "classic" definition of a minority (p. 4) and applies it by discussing the objective characteristics as well as the subjective resentments and "persecution complexes" of white southerners. In their case, "visibility" is of course replaced by "audibility," that is to say, the southern accent (which, incidentally, is rarely acquired by "acculturated southerners").

How the white southerner developed as a distinct social type with characteristics of a quasi minority is shown in a brief but excellent chapter on the social history of the region. Here the emphasis is, of course, on the post–Civil War period, in which the southern inferiority complex and defensiveness developed. In this section the second main theme of the book is taken up: the relations between white and black, particularly since the civil rights movement and white reaction to it and to desegregation. This is followed by a discussion of the changes in the class structure of the South and their causes and consequences (chap. 3); special attention is paid to "marginal" white southerners, that is, Catholics, Jews, and

"transplanted Yankees" (chap. 4). The focus here is not on the fears and hostilities aroused by these "alien" elements but, rather, on their absorption into the regional culture and consciousness. The author then turns to the southern whites in the North (chap. 5). Here the author can rely on his early research (1949) on the Chicago "hillbilly" communities and on his own experience as a transplanted southern intellectual and liberal. Consequently, this is perhaps the most original contribution in the volume. The "hillbillies," as they are commonly called, although only a small part of the southern white blue-collar workers in the North who come from the mountains, constitute a true minority, at least temporarily, and are regarded as such by their neighbors.

The last chapter deals with the political situation in the narrower sense and is centered around the twin issues of states' rights and white supremacy. In order to lend support to the minority thesis, Killian uses the interesting device of comparing the tactics of white southerners and southern blacks.

The book ends with a discussion of the choices open to whites and blacks. While not excluding the possibility of a hardening of polarization, Killian seems to believe that integration may be achieved more easily in the South than in other regions. Recent factual developments seem to give stronger support for this belief than the optimistic statements of the older southern liberals quoted by Killian.

The book as a whole is a competent, understanding historical-sociological treatise on the subject and, beyond that, a fine introduction to many problems of the South—perhaps the best since W. J. Cash's *Mind of the South*. It is well written without unnecessary sociological jargon. The main thesis is consistently followed through and supported by convincing argument. I would question the analogy between the "concurrent majority" theory and the direct action tactics practiced by Black Power groups (p. 128), because Calhoun's doctrine concerned legal matters whereas direct action resorts to extralegal means.

A major question may be raised concerning the author's concept of minority. While it is adequate for the immediate purpose, it is too ethnocentric if this monograph is intended as a contribution toward a general sociology of minorities. The psychological characteristics attributed to minorities are by no means shared by all ethnic minorities. We know many cases where ethnic minority members harbor feelings of superiority against their "host" people and where the minority is held in high esteem by the latter. The German minorities in eastern and southeastern Europe who brought advanced ways of farming or superior craftsmanship and industrial technology to "backward" countries never fitted Killian's concept. The Germans in the Baltic provinces of Russia actually held a dominant position throughout several centuries. The position of German planter and merchant groups in Latin American countries or of Chinese merchants in Southeast Asia are similar situations, though with some modifications. A general theory of minorities will have to differentiate

between those who, like the eastern European Jews, occupy a "pariah" position and those who do not.

The Wahi Wanyaturu: Economics in an African Society. By Harold K. Schneider. Viking Fund Publications in Anthropology, no. 48. Edited by Colin Turnbull. Chicago: Aldine Publishing Co., 1970. Pp. xi+180. $6 95

Peter Rigby

Makere University, Uganda

It is both exciting and dangerous to review a book about a society which is very close, spatially as well as in its cultural and structural aspects, to another in which I have worked recently. (The society to which I refer is that of the Wagogo, who live in the central Tanzania rift immediately to the southeast of the Wanyaturu.) Inevitably, comparisons of which a more detached reader would be unaware spring to mind. But this may also have its advantages, in that detailed comparisons may eventually have considerable theoretical importance in a situation where at least some variables can be kept "constant."

The Wahi Wanyaturu are a "Bantu speaking" people of central Tanzania. They know themselves traditionally as the Arimi but, as with many other East African peoples, came to be known by the appellation given them by outsiders. Little was known about the sociology of the Turu until the publication of the present volume; but several chapters of this book have previously been published elsewhere in substantially the same form.

Professor Schneider adopts as his frame of analysis what he calls "the competitive approach to African society," and he makes the basically "traditional" economic system of the Turu his starting point. He eventually concludes that, at the time of his fieldwork (ending in 1960), this economic system was still "intact," despite the intrusion of alien economic and political forces for the past eighty years and more. Hence, the author makes little distinction between the past and the present in the main body of his analysis, except to point out that institutions have undergone radical change or have entirely disappeared. A concluding chapter enumerates the more obvious changes from precontact times.

In the other eight concise chapters, Schneider deals with all the major aspects of Turu society: ecology, production, the market system, the kinship system and "relations of authority," legal rights and obligations, and, briefly, the structure of the religious and moral community. The emphasis upon competition and individualism, often expressed in aggressive behavior of various kinds, holds the analysis together. Despite his explicit rejection of straightforward "functionalism," the author occasionally falls into the traps he has set out to avoid, such as when he states that, in Turu society, "rituals and other means . . . have been devised to cope with a chronic problem" of instability in the system of production and economics.

Nevertheless, many excellent structural sketches on a variety of problems appear throughout the book, arising from the particular ethnographic contexts. For example, at the end of chapter 3, on the kinship system, the author gives an elegant summary of the characteristics shared by most segmentary lineage systems. In the same chapter, there is also a very good exposition of the "cross-cutting principles" of lineage and sibling solidarity, the latter expressed in the continuing interest a brother takes in the welfare and rights of his married sisters and the consequently critical and delicate affinal tie between sister's husband and wife's brother. Then again, there is the discussion of Turu cattle (and other livestock) as "money," although from comparative evidence I feel that the author pushes the economic precision of the system of livestock exchange a little too far.

With regard to problems of development and planned change, there is a very lively exposition of the reasons for the total failure of "destocking campaigns" during the colonial period, in which the economic logic of the Turu triumphed over the misguided intentions of their colonial shepherds. Here, one regrets the absence of an up-to-date view of the consequences for Turu society of more recent development strategies, such as Mwalimu Nyerere expressed in *Arusha Declaration* and in his later writings, such as *Socialism and Rural Development* and works on the drive for the establishment of *ujamaa* (communal) villages We must await the forthcoming book by Marguerite Jellicoe for an appreciation of the effects of some of these policies on Turu social organization

The author makes good use of folktales as sociological models for particular structural sets at various points in his analysis, for example, on pages 91 and 138. This adds life and "cultural reality" to a book which is occasionally flat and insufficiently illustrated with ethnographic materials. He presents an interesting analysis of Turu notions of witchcraft and sorcery and the contexts of accusation, and I think most anthropologists with experience in this area would agree with his explanation of the famous "lion men" of Singida, about whom some colonial administrators were more credulous than the Turu themselves.

Striking ethnographic similarities and differences between the Turu and their Bantu-speaking neighbors crop up throughout the book. There are, among other things, the unusual verbal distinction the Turu make between male and female witches; the fact that all death is attributable to the malice of others as expressed in witchcraft and sorcery; and the fact that Turu believe, even in abstract terms, that one cannot be bewitched by members of a different village. The careful calculations Turu make about the value of grain and its part in the system of economic exchange are explored in detail, as is the system of livestock transactions and reciprocal aid among close agnates and nonagnatic kin. Schneider's analysis of the network of rights and duties, both individual and corporate, in property and persons leads to the conclusion that the "lineage principle" and the deal corporateness of lineages are not descriptions of reality but provide he "homemade model" of a structure within which individuals actively ursue their own interests.

Despite considerable similarity between Turu and Gogo systems of kinship terminology and action, marriage is highly stable in the latter and unstable in the former, although Schneider gives no exact figures Dorjahn's (1959) contention that, in most polygynous societies in sub-Saharan Africa, only 30–40 percent of married men have more than one wife at a time is borne out by Schneider's material on the Turu ("exactly 33%"), as well as by mine on the Gogo (35.5 percent). The importance of such facts as that the Turu and the Gogo (and others) have cognate words for ancestral ghosts, that they share the formalization of moral but not legal responsibility for kinsmen and affines who are accused of homicide, and that they believe that cattle and sheep (but not goats) have ritual value cannot be overestimated in the analysis of any of these societies. Neither can one ignore the significance of the facts that both Gogo and Turu feel that marriages should not be based predominantly upon romantic attachments and that Turu and Gogo have very different cross-cousin terms.

This brings me to my one major criticism of this book. Schneider makes little effort to explore the comparative implications of his analysis. I do not suggest that every monograph should be an exercise in comparative sociology. But some cross-references to the work of others on similar problems in neighboring societies would have enhanced the quality of the book. And comparative material, which is available in abundance, would have helped, for example, in his discussion of whether or not the word *mbeyu* can be translated as "clan." As it stands, his interpretation clarifies nothing except his own problems in translating Turu concepts into English anthropological terms. Again, comparative references would have been enlightening in his examination of the intriguing institution of *njughuda*, in which women take a dominant role in reordering the easily unbalanced relationships between the sexes. It is clear that some aspects of this institution are influenced by the women's moot and the "fine of women" among the neighboring Barabaig, described by Klima in 1964. Turu have hostile and uneasy relationships with the Barabaig, but the latter certainly cannot be ignored in understanding parts of Turu social structure.

Similarly, the lack of comparative references renders Schneider's explanation of the use of the prefix *wa-* in Turu kinship terms (p. 50) completely unintelligible. I also wonder whether his use of the term "millet" for what appears sometimes to be different varieties of sorghum is legitimate, since he makes no use of Rounce's excellent book (1949) on the agriculture of these areas.

The epistemological status of some of Schneider's theoretical assumptions appears vague, as when he claims that "it should be possible to study an economy like that of the Turu *as a single system of material, service, and social transactions*" (my italics). And what is one to make of the statement that the Turu kinship system "can be seen as under immense pressure to reconstitute as a system . . . of segmentary lineages"? If the "system" itself is derived from Turu norms and behavior by the anthro-

pologist, where does the "pressure to reconstitute" come from and how is it experienced?

Proofreading in this otherwise admirably produced book is sometimes sloppy. Pospisil (1963), Belshaw (1968), and Massell (1963) all appear several times in the introduction but not in the bibliography. The map reproduced on page 9 is nearly illegible in parts, and there are tables in the book which are not listed in the table of contents.

Despite these minor quibbles, however, this is a very valuable addition to the ethnography of central Tanzania. It also represents a complete justification, on theoretical grounds, of a mode of analysis advocated by an anthropologist who has already contributed greatly to the field of economic anthropology.

Tradition and Economic Progress in Samoa: A Case Study of the Role of Traditional Social Institutions in Economic Development in Western Samoa. By David Pitt. Oxford: Clarendon Press, 1970. Pp. xi+295. £3.00.

Lowell D. Holmes

Wichita State University

It has been said that, since man is born without instincts and must learn his values, attitudes, and patterns of behavior from other men, much of his life is oriented by either myth or misconception. Nowhere is this statement more true than in the interpretation of economic activities on a cross-cultural basis. The popular myths of the "lazy primitive" or the economically irrational native have been enduring stereotypes in spite of volumes of counterevidence which have been produced by Malinowski, Firth, Herskovits, Goodfellow, and others.

Tradition and Economic Progress in Samoa is a case study designed to explore the validity of a variety of "negative models" which are commonly held by Europeans concerning Samoan economic values and behavior. These models, frequently associated with peoples outside the Western tradition, include misconceptions such as (1) Samoans prefer leisure to constructive work, (2) Samoans receive no economic incentive from their traditional society, since despotic hereditary chiefs and extended families claim all of a young man's earnings, (3) Samoans prefer consumption to capital accumulation for future production because they are incapable of deferring gratification, (4) Samoans occasionally think logically, but when that occurs it is achieved through rules learned by rote rather than by using principles of reason, and (5) Samoan communal organization is generally incompatible with economic progress.

The theoretical frame of reference for Pitt's study is "to apply a different explanatory model to a situation of underdevelopment through a study of all elements contributing to Western Samoan economy and society." Thus, there is a necessity to deal with two social entities—Samoan and European—and with two value systems and modes of

behavior, *fa'asāmoa* (traditional Samoan) and *fa'apālagi* (Western). Apparently, the success of both Samoan and European enterprises is dependent upon the nature and extent of input emanating from the representatives of the two factions.

Investigation of this economic situation entailed field research for approximately one year, during which Pitt observed social, political, and economic aspects of life in two rural villages on the island of Upolu and in the port town of Apia. In the village of Malie, nine miles from Apia, economic activities centered about cash cropping and wage labor, while in the more remote village of Salani (a four-hour bus ride from Apia), the people were essentially only cash croppers. The Samoans in Apia, the locus of most of Western Samoa's governmental and commercial activities, were almost exclusively wage laborers.

The methodology of the study was the one traditionally employed by anthropologists—observation, interview, and analysis of government, mission, and commercial documents. Quantification is used wherever useful and appropriate. Particularly valuable in the documentation of Samoan economic values and behavior are tables revealing the nature of consumption of necessities and luxury goods, daily work schedules for males (titled and untitled) and females, amounts and kinds of goods exchanged on ceremonial occasions, occupations of Samoan wage laborers, and the nature and amounts of savings, investments, and loans of Samoan villagers.

The study has historical depth and treats Samoan economics within the total cultural context. Although some minor points having to do with alleged modern polygyny and with interpretations of rank involving the status of chiefs (*ali'i*) and talking chiefs (*tulafale*) in the village social structure may be questionable, the monograph is essentially sound and insightful. It should be required reading for anyone engaged in or contemplating governmental, missionary, or commercial service in either Western or American Samoa.

Rather than describe Pitt's treatment of each of the "negative models" cited earlier, it is sufficient to note that in general the idea that traditional institutions cannot promote economic progress is proven to be untenable Whether in the recruitment of labor in cash cropping, the cooperation of relatives in trading enterprises, or the encouragement of commercially minded young men by families who look to them as potential recipients of important chiefly titles, the traditional institutions of Samoa provide support in every instance.

Of special interest to anyone interested in cultural dynamics in developing nations is David Pitt's final conclusion that Samoa, and perhaps other Afro-Asian societies as well, is underdeveloped only in a relativist sense and that normally applied standards of level of consumption are an inadequate index of wealth or economic satisfaction. He suggests that each and every society may have its own concepts of proper consumption and that wealth gained at the expense of the destruction of traditional structures and values is not worth the effort. It may very well be that

Samoans are not the only underdeveloped people in the world who share the sentiment that "they would be wealthier if they move outside the orbit of *fa'asāmoa* [traditional institutions] but few think they would be happier."

Traditional Balinese Culture. Edited by Jane Belo. New York: Columbia University Press, 1970. Pp. xxvii+421. $20.00.

Hildred Geertz

Princeton University

If you are looking for a society whose modes of functioning are so divergent from those of the West as to provide a maximally demanding test of the precision and validity of your favorite sociological schemes, let me propose Bali. It is not a place for the intellectually fainthearted, however, or for the methodologically meticulous. Its major drawbacks as a proving ground are also its strengths, being outcomes of the very complexity of Balinese society. Neither primitive nor simple, Bali has a kind of structural baroqueness and cultural subtlety that, in some senses, can stand up as a contrast to an industrial society. But the very intricacy of its forms is so baffling that the incautious researcher runs the danger of being seduced from the resolutely comparative stance into a fascination with Bali as such. Concepts have a way of becoming Bali centered and Bali relevant, the object itself becoming the subject of study rather than its vehicle. The temptation here is not the mythic one of "going native" but of finding the data itself so engrossing that you will never return to theory building.

For the seducible, *Traditional Balinese Culture* provides an elegant introduction to the extensive literature on the Balinese. A compilation of papers from the 1930s and 1940s that have been buried in inaccessible journals, it represents the responses to Bali of a group of American scholars and artists, people of varying degrees of sociological sophistication but all of penetrating aesthetic sensibilities and long personal experience in Bali. The concern of these writers is mainly with the arts—music, drama, and dance—and with Balinese interpersonal relationships, which are themselves a form of art. It should be read, for balance, together with the two recently issued collections of Dutch research of the same period (*Bali: Studies in Life, Thought and Ritual* and *Bali: Further Studies in Life, Thought and Ritual,* edited by J. van Baal et al. [The Hague, 1960 and 1969]), which stress social, ritual, and economic organization. Taken as a whole, these sets of early papers form a solid foundation for the understanding of more complex works such as Gregory Bateson and Margaret Mead's *Balinese Character* (New York, 1942) and Jane Belo's *Trance in Bali* (New York, 1960).

There are four pieces here by Belo herself—on twins, on the Balinese "temper," on children's drawing, and on Balinese family organization—all

insightful and authoritative. The rest of the book comprises four of Margaret Mead's lesser (and unrepresentative) articles on the arts and children; two by Colin McPhee on music and dance; an unusual text giving a verbatim explanation by a priest of Balinese religious entities, translated by Katharane Mershon; an essay by Beryl de Zoete and Walter Spies on dance and drama; a "blind" psychological analysis of children's drawings by Theodora Abel; two pieces by Claire Holt (one of these, on the dance, is with Gregory Bateson); and two others by Bateson.

In a volume filled with evocative ethnographic description, nuanced, perceptive, and meticulously accurate, Bateson's articles stand out for a different virtue: their bold conceptualizations of the processes of cultural symbolizing. These are the most important essays in the book, because they represent a more general inquiry into the formal characteristics of culture. The paper "An Old Temple and a New Myth" especially deserves rescue from oblivion, for it sets out the peculiar Balinese manner of apprehending historical change, continuity, and causation and relates these to the general processes of myth making and ritual action. Bateson's paper with Claire Holt, "Form and Function of the Dance in Bali," is less developed but gives an intriguing sketch of some major formal dimensions that a full contextual study of dance as culturally expressive behavior might need. Better known, perhaps, is "The Value System of a Steady State," a valiant, if not entirely successful, attempt to bring together Bateson's interactional concept of schismogenesis with his culturological one of ethos via the psychogenetic model that he and Mead developed in *Balinese Character*. It fully deserves another reading in the light of more recent theoretical developments.

The collection altogether, despite the heterogeneous backgrounds of the authors, has a certain consistency of outlook and subject. An attentive reader can construct a fairly complete picture, not, of course, of Balinese society as a whole, but of the major concerns of, as the title states, traditional Balinese culture. In particular, a student of the social and psychological contexts of creativity, innovation, and conservatism in art and ritual would find very little in this book that is out of date and much that is highly provocative. But he runs the risk of being drawn into the Balinese web and, if he emerges again, of seeing his earlier conceptualizations in shreds about him.

Neighbors: The Social Contract in a Castilian Hamlet. By Susan Tax Freeman. Chicago: University of Chicago Press, 1970. Pp. xviii+233 $10.00.

M. J. Meggitt

City University of New York

Given the kinds of social changes that have been overtaking tribal and peasant societies around the world during the past few decades and the

rate at which they are proceeding, social anthropologists nowadays seem to be concerned more and more with devising ways of investigating and trying to account for social systems more extensive and more complex than those which formerly engaged their attention. Consequently, it is interesting to encounter an ethnography that presents a detailed analysis of the social structure and cultural values of a rural community numbering between eighty and ninety people and treats the interrelations of this hamlet with the handful of slightly larger communities that make up a small geographically, and perhaps ecologically, differentiated region in the northeast of Spain.

The author, Susan Tax Freeman, makes a case for her choice of subject matter on two grounds. First, she points out that ethnographic and ethnological investigations of the late nineteenth and early twentieth centuries, especially by Joaquín Costa and his associates, revealed the prevalence of such communities, particularly throughout the north of Spain. Therefore, in order to gain a clear picture of Spanish settlement patterns and local social structures, it is necessary to understand the characteristics and functions of these small-scale communities. This, she argues, is even more necessary because few investigators since Costa have bothered to pursue these inquiries. Rather, interest has fallen on communities such as southern agro-towns that are more diversified internally. More attention has been paid to hierarchical social structures in these situations at the expense of examining the nonhierarchical, horizontal organization and egalitarian traditions that the author believes are also a fundamental feature of Spanish social life. Hamlets such as Valdemora, she asserts, provide an appropriate setting for such inquiries.

Within the limits of these assumptions (which are reasonable and testable), the author has produced a gracefully written and clearly focused account of an enduring rural community that is a valuable addition to the ethnographic literature on Spain. She has been fortunate in having access to archival and other historical material going back to the sixteenth century, so that she is able to indicate both changes and lines of continuity in Valdemora with respect to demographic features (including migration), economic life, and relations with the church and with the Duchy of Medinaceli, to which the Valdemorans and their neighbors owed dues and taxes of various kinds on their agricultural, pastoral, and silvicultural activities.

The author also uses these records along with her own genealogical and other field data to analyze changing patterns of emigration and intermarriage and thus to indicate the varying networks of social interaction that have linked Valdemora with neighboring villages and with the larger towns of the region. The demonstration of the shifting back and forth of family names among the villages as a result of marriage choices is both neat and methodologically useful.

Finally, the detailed discussion of the slow but significant changes that have occurred during the past twenty or thirty years in the corporate jural and economic areas of Valdemoran life provides evidence for the

kinds of systematic interconnections that are usually assumed to exist among the sociocultural institutions of small-scale communities.

In short, the author has succeeded in what she set out to do. The book (handsomely produced, by the way) will not only interest those concerned with Spanish ethnography—social anthropologists generally should read it with profit.

Learning to Be Chinese: The Political Socialization of Children in Taiwan. By Richard W. Wilson. Cambridge, Mass.: M.I.T. Press, 1970. Pp. xiii+203. $10.00.

Nancy J. Olsen

Stanford University

The often baffling and seemingly bizarre political behavior of the Chinese people can be understood, Richard Wilson asserts, by analyzing the way in which Chinese children acquire attitudes toward authority, group cohesiveness, acceptable targets for aggression, and other nonpolitical but "politically relevant" aspects of personality. To gather data on this subject, Wilson studied three large primary schools in Taiwan—two in the capital city of Taipei and one in a nearby rural area. His methods included observation of classroom teaching and pupil behavior, a content analysis of school textbooks, a questionnaire designed to get at attitudes toward authority and decision-making processes (e.g., "Who is the head of your home? What do you feel about this person?"), unstructured interviews with a smaller number of children on the same subjects, a TAT-type projective test, a test of cognitive images of government, and interviews (the content of which is not described) with an unspecified number of educational authorities, teachers, and parents. From this welter of information, evidence is selectively drawn to support what is essentially a culture and personality argument.

Wilson's theory, in brief, is that Chinese child-rearing practices rely very heavily on shaming techniques that involve the threat of denying love. This training is begun at any early age, and the result is a deeply internalized identification with and fear of rejection by "the group" Since the leader both represents the ingroup to outsiders and serves as a model for the behavior of his followers, the only appropriate attitude toward the leader is one of uncritical loyalty. Feelings about authority figures originate in the family context. Modern Chinese fathers are affectively involved with their children and use psychological techniques of discipline, with the result that children learn to identify with and have warm feelings toward leaders in general. The behavior of the primary school teacher parallels that of the parent, and in this and other ways, learning in school reinforces that which occurs in the home.

Shaming, while producing conformity and group loyalty, also produces insecurity, doubt, fear, and inner rage. These potentially explosive feelings

of hostility are released against sanctioned targets outside the group—foreigners, Communists, traitors—and are also expressed in widespread political cynicism and in a careful distinction between the correct external form of behavior and one's own inner feelings. The political results of this process are there for all of us to see—intense loyalty toward and inability to criticize leaders, hatred of outgroups, acceptance of corruption and cheating, the masking of inner feelings by polite behavior, etc. (the reader is invited to supply his own personal stereotype of Chinese social behavior).

This approach to the study of "national character" has been out of favor among sociologists and anthropologists for many years. Readers of this *Journal* need not be told that illustration is not evidence, nor will they be impressed when the selected examples are in the form of tables significant at the .01 level (particularly when the statistical test used to produce this figure is nowhere mentioned). Rather than dwell further on the method, then, I would like to take up Wilson's argument itself and, particularly, to comment upon two closely related issues—first, since it is pivotal to the whole scheme, whether parents in Taiwan in fact rely more on shaming and withdrawal of love than on the "direct" techniques of discipline and, second, Wilson's apparent insensitivity to social differentiation in Chinese society.

Anthropologists who have lived in Taiwanese villages are unanimous on the first point. The direct forms of punishment are the ones most commonly used by rural parents. Both Norma Diamond (*K'un Shen: A Taiwan Village* [New York, 1969]) and Margery Wolf ("Child Training and the Chinese Family," in *Family and Kinship in Chinese Society*, edited by M. Freedman [Stanford, Calif., 1970]) describe slaps and beatings, scolding, cursing, threats of various sorts, and occasional food deprivation. But neither ethnographer observes the warm parent-child relationships which Wilson reports. Fathers in particular were considered frightening and unapproachable. Physical punishment was apparently not as frequent in the village studied by Bernard Gallin (*Hsin Hsing, Taiwan: A Chinese Village in Change* [Berkeley, Calif., 1966]), but threats and scoldings were commonly used. Gallin also agrees with Wolf and Diamond in characterizing the father-child relationship as formal and distant.

Wilson refers to none of these studies, nor are we told how he arrived at his conclusions about the prevalence of shaming and the denial of love as a disciplinary technique and the increasing intimacy between father and child. One suspects that Wilson's informants may have been those with whom he had social contact, probably among Taiwan's educational elite.

This brings me to my second point, which is that modern Taiwan is a very complex society. One-half of the adult population is now engaged in nonagricultural occupations ranging from traditional, low-status peddlers and laborers to Westernized, highly educated professionals. Mainland refugees comprise 15 percent of the population, and there is a definite, though less pronounced, cleavage within the native Taiwanese group be-

tween those whose ancestors came from Fukien and Kwangtung provinces —the Hokkien and Hakka peoples, respectively. There is no reason to believe that social class and rural-urban residence, which have proved to be such powerful predictors of child-training differences in the United States, would not also affect parent-child relationships in Taiwan. (My own research shows that they do.) It is less self-evident, but certainly possible, that differing child-rearing emphases would characterize mainlainders and Taiwanese (many Taiwanese I talked with believe this is the case), mainlanders from different provinces, and Hakka and Hokkien groups. Yet throughout Wilson's book, we are simply told how "Chinese parents" behave, and the way they behave is to use shaming and denial of love. For the vast majority of families on Taiwan, this, is, I believe, just plain wrong

If the political attitudes of children on Taiwan are not to be explained by the type of discipline they have received or their feelings toward their fathers, how were these attitudes acquired? The answer is, I think, a simple one. The children say that Chiang is benevolent and brave, that they should trust their leaders, that the Communists are "bandits," and all the rest of it, for the same reason they say $2 + 2 = 4$—because that's what their teachers tell them. In a fascinating appendix, Wilson describes the centralized governmental control of curriculum, textbooks, and teaching methods, which is reinforced by periodic visits of inspection and classroom performance on centrally set exams. Because it is free of commentary, this is the most compelling part of the entire book, and it makes one realize what a fine contribution Wilson could have made had he chosen a slightly different goal. From his extensive knowledge of the Taiwanese educational system, he could have fashioned a very valuable monograph on the way in which a government which considers itself under attack attempts to consolidate its power by inculcating appropriate political attitudes in the young. In fact, a good deal of this material is available in various sections throughout the book. The reader who has the perseverance to ignore the national-character argument while searching for this more descriptive material could be rewarded with considerable insight into what political socialization in Taiwan is really all about.

Social Character in a Mexican Village: A Sociopsychoanalytic Study. By Erich Fromm and Michael Maccoby. Englewood Cliffs, N.J.: Prentice-Hall, Inc , 1970. Pp. xv+303. $8.95.

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The aims of the authors of this innovative study are "to test a new method which permits the application of psychoanalytic theory to the study of social groups without psychoanalyzing the individual members of the group, . . . to test the theory of social character, . . . [and] to discover data which might be useful for prediction and planning of social

change in peasant society" (p. 226). The research, carried out in 1958–63 in a village of 800 near Cuernavaca (and, incidentally, near Tepoztlán), is the joint product of the authors, of Mexican medical doctors and psychiatrists, and of the anthropologists Theodore and Lola Schwartz; however, the theoretical framework and data analyses are the work of the authors alone. Most description and analysis are based on an "interpretative questionnaire" (reproduced with coding instructions in the appendix) administered to 95 percent of the adult population ($N = 406$), plus sixty Rorschachs and sixty-six Thematic Apperception Tests administered to subjects drawn from the larger sample.

The behavior underlying "social character" is found to be strikingly similar to that of Mexican peasants described by anthropologists: people are reserved and withdrawn, reluctant to form deep friendships, distrustful, suspicious of the motives of others, fatalistic, fearful, and lacking in confidence in their ability and power. But this study goes beyond anthropological analyses by showing that not all villagers combine the same traits and that through social character analysis there emerges a typology of personalities which elucidates such problems as alcoholism and which points the way to prediction about change. Fromm's construct of "social character" postulates that, through social processes, human energy not only is structured in terms of Freud's dynamic character concept, but is structured in culturally patterned ways, so that most members of a society share a common structure which motivates them to attempt to fulfill their social-economic functions with an optimum of energy and a minimum of friction. Since social character, which develops as a syndrome of character traits, is an adaptation to the economic, social, and cultural conditions the group experiences, once the range and variety found in a group is known, its potential for response to changing conditions can be predicted with some degree of accuracy.

Fromm's theory of character differs from that of Freud in that its base is seen not in various types of libido organization, but in terms of a person's "relatedness" to the world. Relatedness is conceptualized via the ideal types of "productive" and "nonproductive" orientations, combined with "receptive," "exploitative," and "hoarding" tendencies, which combine to make four principal social characters: nonproductive-receptive (numerically most common), productive-hoarding (next most common), productive-exploitative (about fifteen cases), and unproductive-exploitative (numerically relatively unimportant). Nonproductive people view all good as lying outside themselves, beyond their creative powers; receptives await passively for whatever may come their way; while exploitatives take by guile or aggression. Productive people are creative, outgoing, active, capable of giving as well as receiving love—the exploitatives much more so than the hoarding types, who find security in conserving and withholding rather than aggressively producing more. Considering both dominant and secondary traits (for obviously, no one fully corresponds to any one type), 71 percent of the villagers are receptive, 55 percent hoarding, and 26 percent exploitative. The nonproductive-receptive peasant corresponds

roughly to the popular stereotype of the listless Mexican peasant, the productive-hoarding to the "anthropologists'" peasant, while the productive-exploitative peasant is the raw stuff from which an entrepreneurial class is formed.

Since, as is pointed out, social character is adaptive to the conditions of life, the presence of three basic types in a single village (presumably duplicated in many other Mexican villages) has important implications for contemporary Mexico. The nonproductive-exploitative people are primarily landless day laborers (from whom an urban factory and services population could be drawn?), the productive-hoarding people are free landowners (the political base of a sturdy yeoman class?), while the productive-exploitative group, because of its future orientation, is the key to social change based on what the authors call "social selection," a concept suggestive of the biologists' "gene pool." Thus, the first two types are best adapted to traditional rural Mexico, while the third type is deviant and ill-adapted to this life. But in developing socioeconomic situations, the previously deviant productive-exploitative group now proves best adapted to the new conditions, and its members become economically successful and the leaders.

Although for sociologists and anthropologists *Social Character* has shortcomings, especially the lack of detailed structural analysis of the community, it is a major theoretical and factual contribution to the knowledge of Mexican peasant society which cannot be ignored by those concerned with peasants, their character, and the implications of character type for socioeconomic development. It is a study which should be replicated in other classical peasant societies, such as those of Greece and India.

Sweden: Prototype of Modern Society. By Richard F. Tomasson. Studies in Modern Societies. Edited by Dennis H. Wrong. New York: Random House, 1970. Pp. xv+302. $3.95 (paper).

Bengt Abrahamsson

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Remarkably, Swedish sociology up to the present has not produced any books on Sweden; there have been no broad, holistic attempts, in the vein of Robin M. Williams's *American Society* (New York, 1970), to deal with Swedish society as such, although there are a variety of books on various isolated aspects (e.g., the family, organizations, labor-management relations, and social mobility). Instead, scholars and journalists from abroad have provided the Swedish public with broad description and analysis of its social habitat. Some examples are Marquis Childs's *Sweden—the Middle Way* (New Haven, Conn.: Yale University Press, 1961), Dankwart A. Rustow's *The Politics of Compromise* (Princeton, N.J., Princeton

University, 1955), and Fred Fleisher's *The New Sweden* (New York: David McKay Co., 1967).

One possible explanation of the reluctance of Swedish sociologists to survey, analyze, and explain the workings of their own society from a broad viewpoint might be the rationalism and strongly empiricist climate which has prevailed in the social sciences in the post–World War II period. Indeed, this is one of the characteristics of contemporary Sweden which receives special emphasis in Richard F Tomasson's work. This climate has been averse to large-scale theorizing and speculation; as Tomasson emphasizes, "Swedish scholarship stays close to the fact and is reluctant to generalize" (p. 273). A certain amount of speculation appears unavoidable in a broad societal analysis. Such an approach carries a certain risk of being met with ridicule and disdain in a scientific setting which puts a high premium on strict models, measurability, and prediction. The typical research item of Swedish sociology, at least up to the mid-1960s, has been the survey; as Gosta Carlsson has observed, Swedish sociology has almost come to be defined by its reliance on survey techniques.

However, as a result of this tradition, there are now a rich variety of data on Swedish society which Tomasson has utilized well. The book abounds in detailed information on such topics as newspaper circulation, birth rates, prestige ratings of occupations, income tax rates, and the social backgrounds of members of the *Riksdag*, big businessmen, and higher civil servants. The author deals with the state and politics, the religious situation, the school and university systems, the family, social stratification and mobility, and dominant Swedish values.

My objections to Tomasson's book refer less to what he is saying than to what he does *not* say. First of all, he ignores or treats only very cursorily such important areas as social welfare legislation, labor-management relations, and the mass media. Although he recognizes the welfare system as one of the bases of the comparatively tranquil and rapid modernization of Sweden, he does not demonstrate any interest in that system's historical background, particularly its roots in the labor movement. At least to a Swede, it is hard to understand how one can separate today's welfare system from its political aspects, particularly after the party cleavages over the supplementary pension system during the late 1950s.

Labor-management relations are dealt with primarily in the chapter on organizations. The discussion is restricted to a highly conventional, descriptive account of the size of the organizations, plus a short sketch of the main provisions of the 1938 Saltsjöbaden Basic Agreement between the Swedish Federation of Trade Unions (LO) and the Confederation of Swedish Employers (SAF). I believe Tomasson would have done better if he had engaged in a more critical examination of the *internal* policies of the LO and if he had considered some of the radical discontent voiced against the trade union movement. The author mentions, in passing, the New Left criticism which "centers around the pragmatic materialism of

the LO and their loss of socialist goals—the same criticism leveled at the Social Democrats" (p. 251). If Tomasson had taken the pains to examine this opposition, he would probably have been more careful in applying the end-of-ideology reasoning—which is his main thesis—so strongly to Swedish society.

With regard to the mass media, Tomasson justly points out the social significance of the large circulation of quality newspapers, such as the *Dagens Nyheter* and *Svenska Dagbladet*, but forgets the impact of television. The intellectual stigma so often attached to television viewing in many other countries is hardly apparent in Sweden. More than in most other countries, television (noncommercial) functions as an important source of information, debate, and social criticism (and even some entertainment).

Consensus, end of ideology, and similar concepts are used frequently by Tomasson to describe the prevailing climate in Sweden today. He maintains, rather categorically, that "few would argue with the contention that the stability of Swedish democracy and the extent to which it operates by compromise and consensus is exceeded by no other modern democracy" (p. 55). Recent developments, however, have contributed to shatter this image in a highly dramatic fashion, as exemplified by the Kiruna mining strike in the winter of 1969–70, the wave of wildcat strikes following it, the labor conflicts involving the state and higher civil servants in 1971, and various instances of forceful student opposition. Tomasson emphasizes in his preface that the pace of social change is rapid and that the book cannot be wholly up to date. I certainly do not want to blame the author for not having made these predictions (after all, sociology is no crystal ball); but I believe that a better consideration of scholarly works *criticizing* the consensual position (good Swedish examples are Göran Therborn et al.'s *En ny vänster* [Stockholm: Rabén & Sjogren, 1966] and Kurt Samuelson's *Är ideologierna döda?* [Stockholm: Bonniers, 1966]) would have led the author to a more careful statement of his leading idea Because of this partial failure, the book seems unnecessarily dated. Some factual information is also out of date and could easily have been corrected· Sweden has a unicameral parliamentary system (the bicameral one was discontinued in 1970); since 1969, Sweden has had two television channels, not one.

In most other respects, however—thanks to frequent comparisons with other countries—Tomasson's book gives a comprehensive, sound, and informative introduction to Sweden.

Influencing the Youth Culture: A Study of Youth Organizations in Israel. By Joseph W. Eaton in collaboration with Michael Chen. Beverly Hills, Calif.: Sage Publications, 1970. Pp. 256. $8.95.

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The ever-growing concern in Western societies (especially in the United

States) with the "generation gap" heightens the relevancy of a study exploring the relationship between the adult world and youth cultures in a new country such as Israel. It is often assumed that young Israelis have found their path toward a responsible and constructive role in their society. Under what circumstances and by which mechanisms has this been achieved? This is the main problem to which this book addresses itself.

After an introduction which is somewhat less than inspiring (consider, for instance, statements such as: "Men are not machines who can be used by an impersonal state for very long without becoming inefficient" [p. 22] or "Countless generations of loving, untrained, and sometimes downright inadequate parents have kept the human race going" [p 21]), the author presents a useful classification of possible strategies that adult-making organizations may use toward youth: the negative, the directive, the co-optative, and the revolutionary (pp. 41–42)

While the first three strategies are quite clear (and represent a rising order of self-determination on the part of the young), the reader might wonder about the term "revolutionary" in this context. Obviously, revolutionary movements may negate or co-opt youth just as much as the Establishment. Thus, the author fails in this case to address himself to the relevant question, What is the strategy? but asks, rather, Who is using it? A more fitting expression for the fourth strategy could perhaps be "identification", that is, the adults are identifying with a youth culture and taking it as their reference group.

Eaton argues on both the theoretical and the empirical levels that the dominant strategy in Israel is the co-optative one and that, by and large, this strategy has been successful. His theoretical argument is derived from Israel's special position as a beleaguered immigrant society These circumstances have increased the sense of interdependence among the segments of the population and intensified the ideological climate into which the youth is co-opted.

The empirical data were collected in a small town called Holon, located in the Tel Aviv urban area (p. 83). Holon's population is far from representative of Israel in general. The investigators tried to even out this discrepancy by adding a special sample of poor children from the slum areas (p. 96).

A useful, although rather crude, trichotomy has been introduced here The subjects were divided into "idealists," "realists," and "detached." The author's definition of the idealists as those "with the highest degree of commitment to assign priorities to the accomplishment of social objectives" (p. 23) seems to be appropriate. However, its operationalization is not explicated.

The author reports an extremely high rate of participation in Israel's youth organizations (over 90 percent did participate for a certain period), but also a high dropout rate (p. 83). The youth organizations are strong compared with parallel groups in the Western democracies but are, nevertheless, quite weak compared with their own past.

In analyzing this decline, the author introduces an interesting idea, namely, the crisis of achievement. The fact that the pioneer generation achieved such a high proportion of their goals in Israel presents a problem to the young Israeli. If he wants to adhere to the ideology of the pioneers, he will have to forego their independent and rebellious way of life. Thus, innovative and even revolutionary rhetoric leads in such a situation to continuity and discipline in actual behavior. This crisis is common to any radical youth culture that has survived for more than one generation. When rebellion becomes routinized, the second generation has the options of returning to the prerevolutionary past, conforming to the by now well-established movement, or escalating toward even more extreme slogans. None of these options is particularly satisfactory. Many of Eaton's conclusions can be read as a report of the various compromises Israeli youngsters and youth organizations have made.

Another lesson to be learned from the book is that the convention of lumping together conformity and conservatism is not always warranted. In some nations, as in Israel, the Establishment is one of the most innovative forces in the society. For many young Israelis, and especially those of Oriental background, to strive for change and modernity implies joining the Establishment.

It goes without saying that the material presented in the volume is of great value to anyone who wants to understand the process of coming of age in Israel, especially since it provides such a thorough description of the various organizations. It has merit as well for the reader who wants to reflect on the more general problems of youth in modern societies, although this type of reader might want to take issue with some of the author's general conclusions.

The Police and the People: French Popular Protest, 1789–1820. By R. C. Cobb. Oxford: Clarendon Press, 1970. Pp. xxi+393. $13.00.

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Richard C. Cobb, author of the monumental *Les armées révolutionnaires* and a volume of fine essays, *Terreur et subsistances, 1793–95*, believes he has written a resolutely antisociological book in this treatment of police, protest, and famine between the Revolution and the end of the First Empire. It reflects "a total rejection of sociology and quantification" (p. xv). He takes repeated stands against the attempts of administrators, generals, ideologues, and the sociologically minded to coerce into convenient patterns the variety and humanity of the ordinary people of post-revolutionary and Napoleonic France. He will not, given his "unsystematic mind," be accused of "writing history that is either comparative or 'scientific,' or of seeking to establish general laws" (p. xix).

"No doubt," writes a reviewer in the *Times Literary Supplement* for

November 27, 1970, "the 'sociologists' and 'quantifiers' will be sharpening their hatchets." But why? Cobb's book is a superb contribution to the academic division of labor. Anyone—even sociologists—interested in popular political mentality, the problem of postrevolutionary order, and the nature of common-man politics in a place and time that produced the first essentially modern dictatorship would do better to ignore these invitations to the war of the schools and draw, instead, upon the many resources of this richly suggestive book.

It is at once essay and thick documentation derived apparently from a larger work in progress. Cobb's account of the Directory's police interacting with the common people cannot fail to set off contemporary resonance: his observations on the assumptions, tactics, and ideology of the police and the middle and lower classes are marvelously detailed. So resolute is his focus at this level that he does not once even mention the name of Fouché, who emerged under Napoleon as the first of the great police ministers whose line has so flourished in our century. Interestingly, the discussion is essentially methodological: if the French police of the time (and, indeed, later) were among the most prolific observers of low life, ordinary life, the political life of everyday man, what is the evidential value of their records? The analysis exemplifies the unity of methodological and substantive problems: the inability of the police to comprehend the unique. An example is the (very contemporary) tendency of the police to ascribe local riotous disorder to the influence of outsiders, the rootless, the conspicuously deviant, which is balanced by their extraordinary knowledge of how daily society routinely works—for instance, the habits of this or that *métier*, region, or village. Police conduct is filtered through the political language of the Thermidorean reaction, a scheme for judging conformity and deviance, which Cobb traces finely through such sources as police manuals of the time.

With a discussion of popular movements after 1793, the scope broadens. Cobb attempts to show that political rhetoric in Paris and other political centers was but faintly related to the popular mentality at local levels, even among sansculottes in their brief and tenuous time of triumph, the year II. There was, indeed, no popular "movement," according to Cobb. (We may speculate that its emphasis in historical accounts reflects in part a retrospective projection from later periods, when mass movements informed by common ideologies became recurrent features of European political life.) In this book, the revolutionary impulse below the middle-class level appears as a matter of local coups carried out by a few militants, often linked by family and trade, aiming at control of a town, a parish, or a commune. Because these impulses were so dominated by minorities, Cobb writes, the "statistical analysis of collectivities: crowds, assemblies, armies—which has its own rather frozen and . . . largely irrelevant rewards—must leave many vital questions unanswered" (p. 204). That may well be the case; however, this observation is more illuminating than the earlier "total rejection of . . . quantification" (p. xv).

More important are such analyses as that of the family mechanisms by

which the popular impulse was preserved, after defeat, in the political heritage of France—a common-man parallel to Schumpeter's program for the analysis of the upper classes and social mobility in terms of family history. Another important analysis is the telling treatment of the weaknesses of popular spontaneity and its painful repression, dispersion, and absorption. This focus on the dreary aftermath of revolutionary "ecstasy" both corrects a long-standing historical and ideological distortion and, more specifically, supplements at a different social level Issar Wolloch's recent account of the Jacobin Club's anticlimactic existence under the Directory and its absorption into the Napoleonic synthesis.

A discussion of the politics of food and food supply invokes many of Cobb's themes at the broadest level. Famine was the constant concern of ordinary people; management of the food supply was the preoccupation of all administrations—*ancien régime*, Directory, Empire—at all levels Such a context profoundly differs from one dominated by class struggles, religious or communal competition, and the clash of debtors and creditors. Its primary reference is geographical, dividing areas characterized by local surpluses (*pays fromenteux*), adequate transport, and links with foreign markets from those of low productivity and insecure or inefficient technology and, of course, towns and cities. Lines of struggle are set by purchasing areas from which population groupings attempt to draw supplies The contest reinforces local concerns, which are stark matters of life and death. Meanwhile, the administrative problems of dissimulation, manipulation, and intervention are clearly different from their modern counterparts The political consequences are finely drawn—among them, a clear disjunction between the revolutionary definition of the situation and those of the common man Robespierre's sneer at the common man's concern with "vulgar groceries" was not matched by a corresponding neglect of the problem by practical administrators. Stalin in 1930 could choose another course.

In the Napoleonic phase—which Cobb treats much more briefly than he does the Directory—a new context emerges: the imperial state drawing upon foreign conquests, more efficient transport, and superior police and administrative apparatus, which foreshadows the new national context in which ordinary Europeans were henceforth to live out their private and political lives. The few parallels Cobb draws with contemporary India suggest a whole range of comparison and contrast—again, more illuminating than his refusal to risk the fate of "So-and-so, who attempted to write an Anatomy of Revolution, [and who] disappeared here without a trace" (p. xix). Those whose styles run to the taxonomic, the conceptual, and the comparative can only benefit from reading this book, whose resources burst the limits of a short comment.

Law without Precedent: Legal Ideas in Action in the Courts of Colonial Busoga. By Lloyd A. Fallers. Chicago: University of Chicago Press, 1969. Pp xi+365. $12.00.

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Prevailing conceptions of law in current works in the sociology of law stress social control and the panoply of subsidiary and related ideas so useful to students of social control—power, deviance, conformity, and stratification. This conceptual apparatus is appropriate to the muckraking tradition in the sociological study of law. It facilitates the debunking of the pretense of blind justice and the myth of the rule of law and permits attention to class bias in justice, the rule of the weak by the strong, and other assorted deviations from purely legal order. Behavioral studies in political science are dominated by another form of debunking tradition. In attacking the simplistic supposition that legal decisions flow immaculately from the logic of the law, the behaviorists search for a simple structure of political attitudes that will account for patterns of judicial conduct.

It is ironic that we must look to anthropological studies of the legal process in non-Western settings to find studies by social scientists who are animated by lawyers' notions of law It is in the best anthropological studies that we find a concern for the construction of normative order and the legal reconstruction of events in order to transform "disputes" into "cases" Students of the legal process must apparently examine far-flung and exotic settings to test the limits of conceptions of law derived from the Western legal tradition which social scientists assume has no bearing on Western legal institutions

This enterprise, though ironic, is nonetheless valuable, and Fallers has provided a splended account of an ongoing system of legal argument. He avoids the worst superficialities of the social-control school of law by recognizing that the specifically legal mode of social control operates through moral discourse. For Fallers, the process that is institutionalized in legal institutions is one of moral discourse directed to social control and dispute settlement. Following the jurisprudential theories of H. L. A Hart, he distinguishes the primary rules of obligation in a legal system from the secondary rules that govern the process of constructing and applying the primary rules in specific instances. It is the existence of these secondary rules that gives some modes of moral discourse a distinctively legal cast.

Fallers observed Soga institutions in the early fifties at a time when the Busoga were subject to the British colonial administration of Uganda. As such, their law was an amalgam of native and colonial sources; but the author emphasizes the indigenous component as it appears in the detailed records of some 150 cases litigated in county courts. The proceedings in these courts rarely involved statements of explicit rules, but Fallers is

able to reconstruct the implicit rules that constituted the raw materials of legal discourse about recurrent problems in Soga life, problems involving marriage and adultery, relations with in-laws, and disputes over land and tenancy. Despite the lack of explicit conceptual structure and despite the differences between Western and Soga modes of legal argument, Fallers concludes that the Busoga did operate within a framework of organized legal discourse. Nevertheless, he does not dismiss as unimportant the fact that the concepts and rules of Soga legal discourse are implicit. Indeed, he argues, to quote him directly, that "the interest of the Soga material for comparative legal studies . . . is that it shows how legal a system of social control can be without overt communication about the application of legal concepts—without precedent or legislation" (p. 312). When the normative materials of legal argument and the surrounding society are relatively static, when legal practice is not professionalized, and when legal institutions are accessible to all, litigation and adjudication can consist in a relatively unselfconscious process, not unlike the playing of a card game; everyone knows the rules, so they need not be cited continuously in order to maintain the game. Fallers believes that explicit secondary rules, which Hart or Levi would take to be essential to an orderly legal process, are better seen as adaptive mechanisms in legal systems that must cope with constant social and legal change. In more static systems, it is possible to be "fact minded," to focus on what happened in the dispute in question and let the moral component of disputation remain taken for granted in the background of the discourse.

The growing body of detailed accounts of litigation in exotic settings is a valuable addition to our knowledge of the social construction of normative order. Fallers's contribution is an excellent, useful, and welcome addition to that literature. It would be salutary if such work could be taken as a model for the study of the legal process in our own society. We need sophisticated studies of ordering as it occurs in the numerous legal settings of contemporary society, studies that avoid oversimplified insistence that law is merely clothing for the will of the powerful, vocabulary for the expression of social attitudes, or a vehicle for the application of arbitrary fixed rules. Fallers's account abjures all these assumptions, and I see no reason for assuming that studies of legal process in modern society cannot follow that example. What will be difficult, as all who have studied the valiant but disappointing struggles of the ethnomethodologists must know is separating the explicit from the implicit rules of moral discourse. It is clear that even the most insistently explicit forms of legal argument assume a massive taken-for-granted background. In this sense, Fallers's distinction between Soga and Anglo-American law is relative, as he himself recognizes (p. 313). The underside of modern legal reasoning is only dimly understood. Further comparative studies of the legal process in the multiple and varied lower-level agencies of law and administration in industrial society might well demonstrate an underlying similarity in the character of legal intercourse in both traditional and modern society.

The Sociology of British Communism. By Kenneth Newton. London: Allen Lane, Penguin Press, 1969; distributed in the United States by Fernhill House. Pp. viii+214. $7.50.

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This study originated in a doctoral dissertation submitted to Cambridge University. It might well have remained in its files. The book is singularly unenlightening.

The study is based on a grand total of twenty-seven interviews with members of the British Communist party. Of these, six were Cambridge undergraduates and only four were members of the working class. Not even half were rank-and-file members; the rest were officials on national, district, or branch committees. Almost all came from the greater London area. Scotland and Wales, traditional Communist party strongholds, are not represented. Of the twenty-seven individuals, there were four married couples. A most peculiar "sample" indeed. The interviews are supplemented by various party materials and statistics which are easily available elsewhere and by biographies and autobiographies of present or former party members.

What is most distinctive about the British Communist party, as of all other official Communist parties, is the fact that it forms part of an international movement to which it is subordinated. The author, however, almost completely ignores this international involvement and takes no account of the fact that the twists and turns of the party line over the years must be accounted for in large part by factors that have nothing to do with the British scene. By treating a basically heteronomous organization as if it were autonomous, Newton bypasses one of its major characteristics and makes it impossible to understand its structure and functions.

The second salient feature of the British Communist party is that it has very considerable strength in the trade-union movement and, in particular, among shop stewards, although, as distinct from many other Communist parties, it is numerically weak and unable to exercise significant influence in elections; it has been a long time since it has been able to send even one member to Parliament. Yet nothing in this book deals with the role of the party in the union movement. This is truly a case of Hamlet without the prince.

Third, the Communist party is a rigidly hierarchical organization; yet the author makes no distinction between interviews with high party officials and rank-and-file members. Here the point made earlier about the "sample" of respondents becomes especially relevant. Even in a less hierarchical organization such as the Labour party, a "sample" consisting, say, of Messrs. Wilson, Jenkins, and Healey, three Paddington housewives, seven Oxford students, and a few miscellaneous London members would look ludicrous.

The bulk of the book is devoted to an examination of the sources of

recruitment of the party, of regional differences in party success, and of the effects of unemployment. But most of the results turn out to be trivial. What is one to make of a table giving the percentage of Communists in different occupational categories showing, say, that 0.09 percent of printers or 0.22 percent of students are Communists? Given the small membership of the party (some 35,000 in the sixties), such detailed breakdown is meaningless. What we really want to know is not that 0 34 percent of miners are members of the party but, rather, why it is that the party has had at times a major political influence in the National Miners' Union despite its small success in attracting the rank and file. As to that, the book remains silent.

When it comes to specific "explanations," the author displays at times a startling naïveté, if one can call it that. He accounts for the relatively high proportion of Jews in the party, at least in part, by the assertion that "Communists and Jews have something in common in that both tend to think in materialistic terms." And he asserts, on the basis of a forty-year-old American study, that "in America it was found that those with income of less than $1,000 or more than $5,000 before becoming unemployed tended to become Fascists rather than Communists."

The book concludes that concepts like normlessness, meaninglessness, marginality, or status crystallization are of no use in explaining the social basis of British communism, except in the case of teachers and Jews. The bulk of the party members, we are to believe, are just ordinary blokes pragmatically oriented toward immediate issues and not much concerned with ideology. They are militants just a bit more in a hurry than the members of the Labour party. Perhaps so, but the author has failed to provide a pattern of proof that carries conviction.

We have a number of good studies of the British Communist party. This is not one of them.

Les phénomènes révolutionnaires. By Jean Baechler. Paris: Presses universitaires de France, 1970. Pp. 260. Fr. 12.

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The domestic conflicts of the United States in the 1960s unleashed a rage of writing about revolution, violence, and political conflict—although not, curiously enough, about war. In Western Europe, much the same revival of intellectual interest in conflict occurred. The French Days of May 1968 especially brought on the impulse to reanalyze the old issues. The first round of writing consisted largely of interpretive descriptions of the conflicts immediately at hand. Now we face the second round: the treatises taking a more systematic look at the whole subject matter (however it is defined) but still strongly influenced by recent conflicts.

Jean Baechler's treatise on "revolutionary phenomena" belongs to the

second round; it bears the marks of an earnest desire to understand May 1968 and the possibilities of revolution that it posed. Baechler includes in revolutionary phenomena all actions which "challenge the social order," especially those in which people consciously place themselves in serious opposition to the existing order of things. He proposes that we make the study of that vast range of actions a discipline to be called "staseology" (from the Greek *stasis*: stop, interruption, turning against). Toward the founding of that discipline, he offers a taxonomy, a handful of concepts and definitions, a way of asking questions, and a large number of disparate reflections.

Since no general arguments or findings flow from the conceptual structure, one can only guess its utility. My guess is that it will not add anything to what we already know. Baechler's general typology of revolutionary phenomena, for example, distinguishes: (*a*) marginalities, including mental illness, suicide, crime, peasant wars, and student unrest; (*b*) countersocieties, including monasticism, banditry, millennialism, and Mafia, and (*c*) political revolution. Each of these bags is both mixed and leaky. The book is more valuable for the comments Baechler makes *en passant*, which are always thoughtful and often penetrating He notes at several points, for instance, the fact that banditry is, to an important degree, a creation of expanding or contracting state power instead of a phenomenon foreign to government. Picking up leads from Raymond Aron, he traces some probable and much-neglected connections between domestic violence and the making of war

For every bright idea, however, there is at least one dubious judgment. Baechler tells us that "monopolistic" political regimes are intrinsically fragile while "pluralist" regimes are not (p. 177), that rapid social change generates more protest-inducing tension than slow social change (p. 186), that crime generally increases with prosperity (p. 186), that Western pluralist political systems are the only ones which tolerate multiple values (p 212), that if aggression does not spill out in war or revolution it will find an expression in increased madness, crime, or suicide (p. 244), and so on. Most of these ideas belong to the folk sociology of our time, but they do not belong in a systematic treatise on revolutionary phenomena.

Father of Racist Ideology: The Social and Political Thought of Count Gobineau. By Michael D. Biddiss New York: Weybright & Talley, 1970. Pp. x+314. $10.00.

George W. Stocking, Jr.
University of Chicago

This somewhat overextended and rather pedestrian "investigation of Arthur de Gobineau's Error" (p. ix) is organized around the thesis that "racist ideology is essentially derived from other and earlier social and political concerns, and that the interdependence of these factors . . . is the

key to understanding the later evolution of his thought" (p. 5). The first part, "The Emergence of Social Pessimism," treats Gobineau's life to 1851, when he wrote his sister Caroline that he was planning to write a "large book . . . on the Human Races" (p. 100). Drawing on Gobineau's writings for the legitimist press and his letters to his friend Alexis de Tocqueville, Biddiss explains Gobineau's social pessimism in terms of the impact of the events of 1848 on a petit-aristocratic mind which, despite its "revulsion against the French Revolution" (p. 43), was until then not totally unsympathetic to "the progressive aspirations of his age" (p. 58). On the other hand, Gobineau's reaction to revolution took several years to develop, and when his pessimism finally matured, it seems to have been rooted as much in the "chasm between his aristocratic desires and the miserable gratification that his world could give" (p. 98) as in his response to a specific political "turning point" (p. 59). The second third of the book is devoted to an explication of "The Theory of Racial Determinism" propounded in the *Essai sur l'inégalité des races humaines*. Suggesting that "Gobineau's racism originates from his revulsion against a a society that had rejected the virtues of nobility and that his social pessimism begins as fundamentally a matter of class-consciousness" (p. 105) —and, incidentally, drawing an analogy to Marx—Biddiss goes on to argue that Gobineau's claim that he developed a "scientific" racial determinism "is not to be taken at face value" (p. 176). Actually, his opposition to "materialism, egalitarianism, democracy, socialism, and nationalism" was the antecedent motive rather than the logical consequence of Gobineau's analysis of the fall of civilizations (p. 172). And yet, because "there is no doubt as to the sincerity with which Gobineau came to maintain his racist philosophy," it is necessary to consider "the effect of the theory upon his subsequent thought" (p. 177)—a task which Biddiss undertakes in the last third of the book, "The Triumph of Social Pessimism." Drawing on the correspondence and published works deriving from Gobineau's disappointing diplomatic career in Persia, Greece, Newfoundland, Brazil, and Scandinavia, as well as on his reactions to political developments in France and on his more strictly literary efforts, Biddiss argues that Gobineau's racial elitism became "harsher" (p. 239) and his social pessimism eventually "irreversible" (p. 244). Rejecting interpretations of Gobineau's later thought which see in it "a certain doctrine of individualism" (p. 234), Biddiss maintains that his racial determinism makes "a doctrine of freedom in any meaningful sense impossible—even for the master-race itself" (p. 243). Nevertheless, although in its own terms his theory could not logically provide the basis for a political program, Gobineau "unfortunately failed" to see how his work might in time "be plundered by racists with an interest in preaching explicitly reformatory doctrines" (p. 260).

Straightforward summary does not do full justice to Biddiss's book. Despite its rather simple thesis and its repetitious argument, despite its frequent explication of the obvious and its labored discussion of "in-

consistencies" in Gobineau's thought, the book does offer some reward to the reader interested in nineteenth-century social thought. Gobineau may have been inconsistent in "defending Bonapartist authoritarianism as a means of combating democratic disorder" at the same time that he opposed "the increase in central control which this policy necessitated" (p. 208). But at another level, the consistency of his opposition to the nineteenth-century philosophy of civilization is quite striking to one whose recent thinking has focused largely on English theorists of progress and evolution. Indeed, in the genealogical romance which he eventually constructed to trace his own family roots back to the ninth-century Norwegian pirate Ottar Jarl, Gobineau embraced a tribal model of society which in progressivist thought would stand very close to the polar opposite of civilization. For the liberal progressivists, tribalism tended to be linked to racial inferiority; for Gobineau, racial superiority could ultimately only really be maintained in a romanticized tribal state.

But whatever the occasional compensations, in the end one must face up to the analytic and interpretive inadequacies implicit in the suggestion that Gobineau's "originality" lies in "declining to provide any solution" to the crisis to which his theory was a response (p. 268)—or in the above-noted "unfortunate failure" to foresee what uses nazism would make of his racial theory. Insofar as such prevision was possible in 1853, Tocqueville offered it to Gobineau: " 'Do you not see inherent in your doctrine all the evils engendered by permanent inequality—pride, violence, scorn of fellow men, tyranny and abjection in all their forms?' " (p 149). To treat the fact that Gobineau rejected such foresight as an "unfortunate failure" seems to me to reduce intellectual biography to a rather futile exercise. But perhaps others may find compensation in the "great therapeutic value in relation to our contemporary racial difficulties, both domestic and global," which Biddiss suggests inheres in "the study of Gobineau and of his errors" (p. 270)

Alienation. By Richard Schacht with an introduction by Walter Kaufman. Garden City, N.Y.: Doubleday & Co., 1970 Pp. lxv+286. $7.95.

Melvin Seeman

University of California, Los Angeles

"Alienation" is a code word, like "law and order." The code communicates more about the user's style and stance than it does about substance. A great deal of effort has been expended in seeking to crack the code, and Professor Schacht's is the most sustained and the most successful of these efforts. I think that Walter Kaufman is correct in suggesting, in an extended introductory essay on "The Inevitability of Alienation," that henceforth "nobody should write about alienation without first reading this book" (p. xiv). He is also right when he argues that serious writers "are not satisfied to label what they like 'swell' or 'groovy' or 'divine';

neither should serious writers be content to call what they deplore 'alienation'" (p. li).

Despite Schacht's conceptual care and Kaufman's cautions, however, I do not expect that the existing codes and practices regarding "alienation" will change very much or very soon. Nevertheless, there are important lessons to be learned from Schacht's thorough review of Hegel, Marx, Fromm and Horney, the existentialists, and the sociologists. Many of these lessons are not new, and some of them ought to be unnecessary, but Schacht is skillful and perceptive across the entire range of issues involved. For example:

1. Though it troubles many to acknowledge the *varieties* of alienation (those, for example, who have found *the* code in Marx), Schacht makes it clear that the word has been plural from the beginning. Thus, for both Hegel and Marx, Schacht shows how alienation is used in multiple senses which are not necessarily congruent: for Hegel, in the sense of "separation" of the individual from the social substance (i.e., social and cultural institutions) and in the sense of "surrender" to it; for Marx, there is both separation and surrender (often confusingly fused) in connection with various discriminable alienations—from one's product, from labor itself, from fellowmen, and from one's self

2. Concerning the *normative judgment* that some find so essential in the idea of alienation, Schacht makes it clear that this is only one of the classic usages and has no special claim either in priority or in custom. Indeed, in his concluding recommendations (not likely to be either well received or followed, in my judgment), Schacht suggests that "alienation" be used in a nonevaluative way. "Self-alienation" is, for him, another matter: he recommends that the notion of a discrepancy between one's actual condition and one's ideal or essential nature be retained—at least, he adds a bit archly, "as long as people continue to find the idea plausible" that there is such a definable nature of man (p. 256).

3. Regarding the idea of *dimensions of alienation*—a syndrome of diverse forms which display a certain unity despite diversity—Schacht offers some cautions for overeager sociologists. He doubts (as I do, and have from the beginning) the utility of thinking of alienation as a "multidimensional" concept—finding no warrant to suppose that the variants which have been labeled "alienation" will be found together in any steady sequence or combination. I have grave doubts about the correlations, factor analyses, and Guttman scaling which now attempt to speak to this question in the sociological literature, my doubts resting more on methodological grounds (e g , suspicion about the "forced" unity introduced by method factors in the measurement process) and Schacht's doubts resting more on logical grounds.

4. On the contrast between alienation as *subjective attitudes versus objective condition,* it strikes me that Schacht emerges as somewhat ambivalent. He seems less than enamored with sociologists' heavy interest in feelings and attitudes but concludes, in his own right, that alienation

ought to refer to the individual's *experience* of separation (p. 251). He concludes that the treatment of work alienation by most sociologists "subjectivizes" Marx (p. 162); yet his own discussion has already shown that the Marxian treatment hardly avoids subjective language or implication. Concerning the prevalent mixture of feelings with objective states, he comments (rightly): "It is hard to imagine a situation more conducive to confusion than one in which the term is allowed to function in both ways" (p. 252); but his recommended usage of "self-alienation" applies *whether or not* the individual in question is aware of the existence of the disparity. I would prefer to be even less confused than that and use "alienation" strictly in its subjective sense (while reiterating that no one ever suggested that situational structures—political, industrial, familial, etc.—are therefore supposed to be ignored, or that judgments about the quality of life are taboo simply because they are no longer smuggled in conceptually).

It should be obvious that I have little fundamental quarrel with Schacht's analysis and much admiration for it. I hope that this book is read widely and that it has the thought-provoking impact it deserves. Meanwhile, I imagine that the use of the code word "alienation" will continue to be simply a way of catching the reader's attention, signaling a domain of investigation, and claiming a particular intellectual heritage. That much is easy enough, the hard part comes in making the signals and the claims translate into analytical work that is worthy of the attention.

The New York Abolitionists. A Case Study of Political Radicalism. By Gerald Sorin. Contributions in American History, no. 11. Westport, Conn.: Greenwood Publishing Corp., 1971. Pp. xiii+172. $9.00.

John L. Hammond, Jr.
Columbia University

The study of the Civil War and its origins, always a favorite pursuit of American historians, has received a new impetus from the search for nalogies to the present situation. At the same time, historians are in-reasingly applying the methods and the theories of the social sciences o the study of the past. Among the many recent works on the period receding the Civil War that reflect the influence of the social sciences oth in method and in conceptual apparatus is Gerald Sorin's *The New York Abolitionists*, an effort to test two theories of the motivation of olitical radicals. As a systematic collection of data from the past relevant o a theoretical problem, it does very well; at using the data to test a heory, it does not do quite so well.

Sorin juxtaposes a "tension-reduction" theory of motivation which rgues that abolitionists suffered frustrations which created maladjust-nents and a desire for martyrdom with a theory which explains the aboli-ionists' motivation by their commitment to abstract goals, according to

which the frustration they encountered in the efforts to end slavery may have increased commitment, but did not create it. He summarizes these two as general theories of motivation and shows how they have been used to explain the abolitionists' behavior—a large undertaking for a brief introductory chapter, in which his presentation of psychological theory is necessarily oversimplified. The "commitment" theory, which Sorin favors, is even less clearly stated than the "tension-reduction" theory, and his strategy is principally to confirm it by default through evidence against the latter.

He attempts to refute the tension-reduction theory with biographical information about the abolitionists. Systematically identifying the 100 leading abolitionists of New York during the 1830s and early 1840s (by taking into account offices held in antislavery societies and in the Liberty party, candidacy for public office, and frequency of mention in the antislavery and general press), he provides brief biographical sketches of the leading fifteen and a statistical composite biography of the 100.

The statistical summary uses the SES variables with which we are all familiar, as well as information on geographic origins, ethnicity, and religious activity. The abolitionists were a highly urban, well-educated group, most of them Yankees, either merchants or professionals, at least moderately successful, and with a strong evangelical connection (68 percent were either clergymen or active laymen). Comparable data are presented both for the general population and for all of New York's congressmen between 1838 and 1845; neither can precisely be said to be a control group, but the bench mark makes the data on abolitionists more useful than they would be if they were presented alone. Sorin's argument is that since such men—successful and evidently well integrated into their society—led the abolition movement, it is likely that they "suffered no more or different frustration than that which human beings tend to experience, and were therefore, in their militant behavior, simply responding normally to a challenge to their particular vision of America" (p. 18).

The more detailed biographical sketches describe the path by which the top fifteen became committed to the movement. Despite a diversity of social origins, these men were almost all successful in other pursuits and actively religious before becoming abolitionists (generally in middle age). Evangelical fervor seems to have been a common stimulus to interest in abolitionism, but active commitment arose only after a tentative commitment in the course of which some frustration was encountered.

Sorin fails to distinguish between the level of confirmation provided by the statistical data and that provided by the biographical data. For example, his demonstration that the leaders were not downwardly mobile and that their professions were not losing status in the prewar decades successfully refutes the status-frustration hypothesis (advanced by David Donald and, more recently, by Lipset and Raab). But it deals with only one possible source of frustration: neither these facts nor the fact of the abolitionists' generally high status precludes the possibility that they

projected other internal frustrations onto the public arena in becoming active radicals. The fact that many of the top fifteen were successful in their professions and involved in benevolent activities before becoming abolitionists provides a stronger refutation, but the bearing of specific items of data on different aspects of the theory is not shown with any precision.

Further, although Sorin finds five blacks among his fifteen leading abolitionists, he does not dwell on the important differences between them and the whites: though all were free either at birth or from an early age, they were not from high-status families; they encountered discrimination repeatedly; and they were less likely to have independent careers prior to becoming active abolitionists. Sorin's discovery of such a high proportion of blacks among abolitionist leaders by a systematic ranking is valuable information in itself (perhaps understated in the book at a time when most historians portray the abolition movement as an almost exclusively white enterprise). But it is unlikely that the same generalizations can explain the efforts of whites and blacks to end slavery; greater concern for the relation between theory and data would have stimulated closer attention to these differences.

The New York Abolitionists marshals a valuable body of data on a fascinating group of men, and presents it well. Sociologists will find the book inconsistent in its application of theory, like too much of the literature of the "new history." But it stands, nevertheless, as an important contribution both to that literature and to the study of abolitionism.

The New Religions. By Jacob Needleman. Garden City, N.Y.: Doubleday & Co., 1970. Pp. ix+245. $5.95.

Hans Mol

McMaster University

This book provides an extensive description of some of the oriental religions to which an increasing number of Americans are attracted. California seems to be a particularly fertile breeding ground for these movements. "Cults and sects grow like weeds [here]. Gurus abound," says Needleman (p. 36). He relates this to the despair of California, "the despair of people relentlessly getting what they want."

First, there are the Zen Buddhists, who have a monastery on some 500 acres of rugged mountain land about 150 miles north of San Francisco—a former secluded resort hotel. Here the students rise in darkness each day at 4 A.M. to start one of their many sessions of *zazen*—"just sitting." The goal of Zen practice is the experiencing itself (p. 43).

Then there is Meher Baba, who claims to be God personified, the avatar. "I am everything and I am beyond everything" (p. 77). His followers experience an immense and overpowering love that radiates

from Baba and through his disciples. Baba voluntarily ceased to speak in 1925 and died in 1969, but the movement goes on undiminished.

Subud is another movement, with some seventy centers in the United States. It was started by Muhammad Subud from Java, and its central axis, around which everything else turns, is the remarkable spiritual exercise known as the *latihan*. Twice a week, people gather in a large room or hall for half an hour to receive and submit to the power of God. "At full blast, the *latihan* may sound like nothing so much as jungle animals, or maniacal, savage rites, or an eerie convocation of devils and banshees. At other times it seems a deeply religious choir" (p. 107). According to Needleman, the exercise purifies the individual of animal energies and provides "a new and uniquely 'easy' way for men to be 'injected' with the highest energies of the universe" (p. 110).

Transcendental meditation (Maharishi Mahesh Yogi) also seems astonishingly easy. All one has to do in order to be happier, as the Maharishi said, is to sit comfortably for a few minutes every day and silently repeat a special sound or phrase called a "mantra." Naturally, this "instant nirvana" did not escape the authorities on Yoga and Eastern religions, who "quickly lined up against the gentle guru with the thunderous following" (p. 133).

Krishnamurti teaches instantaneous self-observation, or how to live without inner disorder. He has not much time for teachers, leaders, organizations, and systems of thoughts or belief and rejects a following In his thought there is no God, no religion, no ethnical norms, no life beyond the grave, no new theories or explanations.

Tibetan Buddhism, on the other hand, is centered around the idea of compassion. The Dalai Lama is the incarnation or "node of force" of this idea. Under him in "the hierarchy of stations of energy" are the *tulkus*, Tibetan holy men, several of whom are now in diaspora in the West and, of course, in California.

What does Needleman do with these rather unsystematic and incomplete observations? He is primarily a philosopher (he is chairman of the Philosophy Department of San Francisco State College) and makes no attempt to explain his observation in terms of sociological theories of religion. What Needleman does *not* ignore are psychoanalysis, psychology, and technology: "symbols of astrology . . . provide a potentially far richer framework than our contemporary psychology for approaching people and human relationships" (p. 197). "Western science, technology and the contemporary psychological cult of guiltlessness, among other causes, have greatly dimmed our minds to the demands of our cosmic situation and to the strength of the forces that resist the awakening of consciousness (p 220). "The agony of Western technology is the result of a naive fantasy . . . that [man] need not spend his energy for purposes that are alien to his wish for wholeness and intensity of experience" (p. 216).

However, it is with interesting speculations such as these that Needle-

man tends to rest his case. There is certainly no attempt to provide explanations in terms of social alienation, identity crisis, cognitive dissonance, meaninglessness, disenchantment, etc. And still, his observation that several of these movements have had the effect of taking their followers off drugs could easily have led to higher-level generalizations. Instead, Needleman finishes his book by expressing the hope that these and other religious movements may prevent us from blindly believing in the supreme reality of everything we do, think, or feel at the moment, both as a society and as individuals (p. 230). He also feels strongly that only through religion can civilization function as an intersection between levels of reality (p. 228). It is with points such as these that Needleman is very likely to get the sympathetic ear of many Americans for whom religion is important.

The Mark of Cain: The Stigma Theory of Crime and Social Deviation. By Schlomo Shoham with a foreword by Marvin E. Wolfgang Jerusalem: Israel Universities Press; Dobbs Ferry, N Y · Oceana Publications, 1970 Pp iv+282. $7 50.

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University of California, Davis

The foreword to this work on stigmatization and deviance praises the book as embellished theory, but discerning readers may find that the embellishments obscure rather than enhance the text Much will depend on how they feel about parenthetical observations and digressions into existential philosophy, psychoanalysis, and French autobiography The style creates the impression that the author, in his need to display wit, character, and erudition, may not have given quite enough care and attention to data and logical precision in his presentation of his ideas.

Insofar as it is possible to strip the book down to essentials, it presents social stigmatization as a sociopsychological process of "norm sending and norm receiving." Stigma are regarded as social controls which deter deviance, depending upon whether the norm receivers are "sanction-orientated and other-directed." Norm senders are concerned with the image rather than the reality of societal norms and, likewise, with the image rather than the reality of the victim. Some people get "trapped" by their stigmatic labels; some do not.

One level of analysis deals with factors linked with social stigma, namely, the "predisposition" to get branded. The second level is the dynamic process by which organs of society perform actual stigmatization Predisposing factors break down into value deviation and deviant behavior. Deviant behavior is classified and described as autism, self destruction, bohemianism, accidental deviance, criminality, chaotic rebellion, and ideational rebellion. Value deviation is something like anomie

but more like apathy and a sense of meaninglessness, or "value laden rage" against the "mass that crushes beneath it everything that is different."

The stigmatization process in pure form amounts to victimization in which predisposing factors are irrelevant. Any person is fair game who departs in the existential sense from values and norms of the "solidarists" but who is linked in a feasible way to them and who symbolically embodies the deficiencies and wants of the stigmatizers. The subjective and biased nature of the process is evident in the immunity of lawmakers and rule setters from the sanctions of stigma and, conversely, the vulnerability of the powerless segments of society. The subconscious mechanisms which underlie stigma imposition are transference, displacement of hostility and aggression, and illusory achievement by derogating others.

Pausing to come up for air, we perceive that this is labeling theory in the grand style, replete with a primitive metaphor of scapegoating which is explicated by Freudian psychodynamisms. Its similarity to F. Tannenbaum's dramatization of evil, G. H. Mead's organization of taboo, and H Garfinkel's ceremonies of degradation is obvious, although the author makes only a small effort to integrate his notions with these and none at all with labeling theory as it is currently known in American sociology He does seek to integrate his stigma theory with other criminological theories having to do with family structure, ecological areas, social disorganization and anomie, differential identification, differential association, and opportunity structures. This task, along with formulation of the stigma theory, plus setting aside two chapters for a discussion of psychopathy and of Jean Genet's life, as might be expected, was too much for any man to accomplish in 237 pages of text with any degree of accuracy or rigor. The result is a kind of intellectual tour de force, didactic in tone, but which does not hold together well on close examination.

The author makes an interesting case that the concept of psychopath is an ideal stigmatizing sanction for social deviants and should be so regarded, but he misses an opportunity to pursue the implications of this idea for psychiatrists as stigmatizers. He also finds W. B. Miller's theory of lower-class culture as a generating milieu for delinquency to be a powerful demonstration of his stigma thesis, replicating the less scientific manner by which society at large brands with stigma. Yet the author himself is not above an attempt at stigmatizing when he asserts without documentation that "homosexuality is an organic part of the criminal's world, his folklore and value system" (p. 202).

Sociologists working in the area of deviance can find some interesting and provocative ideas in this book. Moreover, the author properly tries to set the process of stigmatization into a larger theoretical context. To say that he succeeds in any clear-cut way, however, would err on this side of candor.

The Woman Doctorate in America. By Helen S. Astin. New York: Russell Sage Foundation, 1969. Pp. xii+196. $7 95.

Cynthia Fuchs Epstein

Queens College of the City University of New York and Bureau of Applied Social Research, Columbia University

It is important to have this book on women Ph D.'s. There is an appalling ignorance, both in and out of the social sciences, concerning what generates women's capacities for professional work and later maintains their commitment to their careers. Astin's book does much to dispel this ignorance. Tracking the careers of women who received doctoral degrees in 1957 and 1958 and reviewing their family situations, she attempts to piece together the relationship between involvement in work and the demands of family life, presenting the classical dilemma posed between home-centered and occupational roles.

In the tradition of this kind of analysis, Astin's work is a myth-dispelling contribution. She shows that 91 percent of women doctorates are employed (undercutting the popular view that advanced education for women is a poor investment) and that their employment pattern is remarkably stable (one-half have changed jobs only once), damaging another assumption that women are not a good employment risk A greater blow to the mythology of the fickle nature of the female is the finding that less than one-fifth of the fully employed have ever interrupted their careers. These are the striking items, impressive and positive for the defenders of women.

Only the very "good stuff" is reported on the jacket of the book and the strong introduction to it. But a careful look at the data indicates that some of the interpretation was done with rose-colored glasses. The picture is far more complicated, and the record of the women Ph D.'s (as measured by the prevailing male dominant standard) may not be so glowingly interpreted. One might argue that when 91 percent of women with doctorates work and that 81 percent are working full-time, that is very good, but the distribution ranges from a low of 62 percent (the percentage of married women doctorates in natural sciences) to 91 percent (full-time married women doctorates in education). The record of the single women is much higher. Of course, "full-time" in education may actually constitute a shorter workweek than the workweek required of the natural scientist, particularly in research. This may account for the greater percentage in education.

My objection is that the interpretation avoids the special problems women are exposed to when they are working in the male-dominated structure of the natural sciences, when they also choose to lead normal family lives as well.

The view that consistently runs through the book is that women with Ph.D's. are performing rather better than expected, and the answers are to be found in their high ability and motivation to succeed. I have a

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It is important to have this book on women Ph D.'s. There is an appalling ignorance, both in and out of the social sciences, concerning what generates women's capacities for professional work and later maintains their commitment to their careers. Astin's book does much to dispel this ignorance. Tracking the careers of women who received doctoral degrees in 1957 and 1958 and reviewing their family situations, she attempts to piece together the relationship between involvement in work and the demands of family life, presenting the classical dilemma posed between home-centered and occupational roles.

In the tradition of this kind of analysis, Astin's work is a myth-dispelling contribution. She shows that 91 percent of women doctorates are employed (undercutting the popular view that advanced education for women is a poor investment) and that their employment pattern is remarkably stable (one-half have changed jobs only once), damaging another assumption that women are not a good employment risk A greater blow to the mythology of the fickle nature of the female is the finding that less than one-fifth of the fully employed have ever interrupted their careers. These are the striking items, impressive and positive for the defenders of women.

Only the very "good stuff" is reported on the jacket of the book and the strong introduction to it. But a careful look at the data indicates that some of the interpretation was done with rose-colored glasses. The picture is far more complicated, and the record of the women Ph.D.'s (as measured by the prevailing male dominant standard) may not be so glowingly interpreted. One might argue that when 91 percent of women with doctorates work and that 81 percent are working full-time, that is very good, but the distribution ranges from a low of 62 percent (the percentage of married women doctorates in natural sciences) to 91 percent (full-time married women doctorates in education). The record of the single women is much higher. Of course, "full-time" in education may actually constitute a shorter workweek than the workweek required of the natural scientist, particularly in research. This may account for the greater percentage in education.

My objection is that the interpretation avoids the special problems women are exposed to when they are working in the male-dominated structure of the natural sciences, when they also choose to lead normal family lives as well.

The view that consistently runs through the book is that women with Ph.D's. are performing rather better than expected, and the answers are to be found in their high ability and motivation to succeed. I have a

sociologist's resistance to Astin's tendency toward psychological explanation of her findings. There are, after all, many women around with high IQs and managerial ability who direct energies to the success of their husbands and children rather than their own careers. One suspects that there is something more than individual resolve which activates incentive of that tiny proportion of college women who go on to get Ph D.'s.

But Astin's work does indeed provide us with solid and interesting data —at least we have a basis on which to quarrel about interpretations. For example, it is a common view that if women do not succeed it is because they have not put in the spadework that men do—that is, they do not work very hard and do not measure up on the objective criteria of competence, which include, in academia, research activity and publication. Yet Astin claims that women who report experiences with employer discrimination are also the ones who have had more publications to their credit and who have received more honors and rewards for professional achievement than other women. One of Astin's interpretations suggests that these women may be more aggressive, competitive, and frank (characteristics which make one more prone to publish) and therefore more likely than other women to criticize employers. I also catch a hint of a suggestion that they may also be less attractive to employers because of an overdose of these "masculine" attributes. Astin did not acquire data on personality, such as results of personality tests (in their present state of culturally biased loading on the male-female aggressiveness-passivity dimensions, it is probably quite wise that she did not, even if it were possible), so the reasons for discrimination must remain unclear.

The book does provoke some other thoughts, such as the manner in which placement in the opportunity structure, and the view of justice within it, enters into the sense of justice which women are exposed to. Certainly, an attractive part of this book is the number of research leads it offers for further work in this overpolemicized, underinvestigated area of study.

One further item on motivation proved rather interesting as a point to debate: Astin found that high productivity was related to mothers' employment in unskilled or semiskilled occupations. Again, she has contributed to our knowledge by asking about the mother's employment (most studies only ask the father's occupation). Many researchers, Astin included, have found that professional women seem to come from families which are more advantaged than those of comparable groups of men, which would make the likelihood of their mothers' working slim, although a significant proportion do have working mothers. Astin finds a relationship between women working full-time and their having had a working mother, but here, in addition, it was found that the *productive* professional women come from families where the mothers work, but at low-level jobs. In this interpretation, we do not get much information about what the fathers did—whether or not they were even around—and the more subtle but important information about the attitude of the mother toward work.

Rather than accept one of the author's views that "one may speculate that high productivity is a kind of compensatory behavior, designed to overcome role conflicts and inferiority feelings generated by the mother's lowly position" (p. 83), I would speculate that some combination of an economic need to work coupled with the belief that through hard work one can expect mobility in the social structure is a more reasonable interpretation than that women who are high producers are all trying to live down the embarrassment of a blue-collar mother.

In some of my own more methodologically messy research on women professionals, it seems that white women lawyers from poor immigrant backgrounds and black women from disadvantaged backgrounds who work in law and other male-dominated professions view the efforts of their parents to earn a living in tailor shops, factories, and as domestics as heroic acts which instilled a strong work ethic and an obligation to fulfill the hopes of their parents.

But disagreement on interpretation—and I differ on a a number of other points—does not undercut my basic respect for Astin's work, her rigor, and her clear style. Obviously, the questions it brings up cannot be solved within the confines of a single study But now that Astin has set some of the base lines, others can more easily go on with more refined and subtle measures. Astin has given us some important referral points on which we will be able to measure changes in the perspective of women doctorates and the relative opening of traditional institutions to the employment of these women. We have learned from Astin's material that women with specialized training do indeed lean toward using it; many use it and turn in high levels of performance—this in the face of a relatively punishing structure. Astin notes that women doctorates are more productive if they have been at good institutions and have been given postdoctoral fellowships. One might imagine (and, indeed, measure) how much better women might do if the balance were turned toward making the academic structure rewarding.

Profession of Medicine: A Study of the Sociology of Applied Knowledge. By Eliot Freidson. New York: Dodd, Mead & Co, 1970. Pp. xxi+409. $12.50.

Barney G. Glaser

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This is an excellent book on the application of expertise to human affairs. Freidson's point in this book is that "the knowledge of the profession is distinct from the circumstances and conditions in which it is applied, and that while the former may in most cases be said to be the best of our time, and to justify protection against lay interference, the latter is neither codified, systematic, nor objective, reflecting the social position and occupational usages of the profession rather than some special technical

expertise deserving of autonomy." Both the designation of need and the determination of the arrangements by which knowledge is applied are at issue. These, he argues, are the questions on which there is at present no reliable, systematic expertise. How Freidson handles this problem is insightful, alarming, outraging, and agitating, *but* he favors an issue-oriented approach over an analytic one, and like most issue-oriented work his approach favors conjecture about what ought to go on but does not cover what is actually going on. To bolster his issue orientation, his "oughts" are logically handled and laborsomely repeated over and over. Conjecture makes much of what he says sound *sort* of right and *sort* of wrong.

More knowledge of what actually goes on in medical practice might lead Freidson to an analytic approach. It would water down much of his issue making, change some of his ideas, and eliminate some issues that are irrelevant to the concerns of patient and doctor. He leaves out many theoretically and empirically important variables. For example, variations by type of illness—chronic or acute—types of practice, organization, types of doctors, number and kinds of staff involved, family requirements, types of patients, conditions of care—home or hospital, or nearness to medical-center attention—growth of both doctor and patient in their relationship, knowledge and interpretation of illness, degree of pain, cognitive disorientation, etc., of the patient are just a few of the variables which could easily render fairly irrelevant what Freidson considers at issue and the attendant problems. In assuming an autonomous doctor and dependent patient, Freidson misses much of what we know exists in medicine. Thus, Freidson should have specified those conditions in the real world of medicine, where his analysis applies. I can assure the reader that his analysis is not as general as he would wish it. The organization and practice of medicine are far more variable, and so are the issues.

In sum, I agree completely with Freidson's handling of the issues of autonomy of doctors and control of patients, *when his analysis applies*. In the meantime, medical sociology must get on with the task of developing analyses of problems about what is actually going on; the fates of all people—doctors and nurses as well as patients and family—are involved and need these analyses.

Medicine in Three Societies: A Comparison of Medical Care in the USSR, USA, and UK. By John Fry. New York: American-Elsevier Publishing Co., 1970. Pp. ix+249. $7.50.

Mark G. Field

Boston University and Harvard University

This book, by an astute and observant British medical practitioner, is not a systematic description of the medical systems of the Soviet Union, the United States, and the United Kingdom. Rather, it is a series of organized

notes and ideas, peppered with a few easily available statistical data, garnished with some generalizations about the essential features of national health needs and systems of modern societies, and served up as an interesting, often stimulating broth. The author, who is a general practitioner in a London suburb, spent two months in the United States in 1965–66 and five weeks in the USSR in 1967. The main value, of course, is the comparative approach and the fact that the author is a practitioner and therefore aware of the many subtleties of medical practice in the modern world, particularly on the firing line of medicine (general practice and primary care). It is also clear, at least to this reviewer, that Fry's greatest knowledge is of the British health system; that he has some knowledge of the American health picture; but that his understanding of the Soviet system has been gleaned mainly from a World Health Organization traveling seminar. Such a seminar is helpful in providing the visitor with an overview of Soviet medicine and provides a chance to visit some installations. Beyond this, the visitor has to rely on official handouts of the health ministry of the USSR.

The organization of the book is conventional, apart from the comparative perspective. It begins with a general statement: everywhere medical care has common goals and common problems, but each society has national characteristics that, in many ways, mold the manner in which these problems will be met. The author then gives an idea of the structure and organization of medical services in the three countries and gives the reader a chance to compare the tight organization of Soviet medicine with the looser British pattern (and its tripartite division among general practice, hospitals, and public health services) and the American system (sometimes called a nonsystem) with its multiple arrangements. The chapters that follow are on first-contact care, specialist ambulatory care, hospitals, preventive aspects of medical care, public health and social services, maternity and child care, and mental health care. An interesting chapter is on the greater medical profession (all those in addition to physicians who participate in some phase of health services), and this is followed by a discussion of education and training. The final chapter, on present dilemmas, discusses increasing expectations by people everywhere and decreasing willingness of populations to tolerate illness and suffering. These expectations are, to some degree, generated by the application of scientific advances, and although they are common in most countries, the author wisely warns that "no single national system of medical care can be applied to every nation but there are principles within national systems that can and should be applicable to others" (p. 228). One common dilemma is a demand for services that exceeds any nation's capability to satisfy. Nations must not only assess what they want to do in medical care, but also their resources (financial and manpower) in the light of many competing claims from other systems. It has also become clear everywhere that the traditional idea of the individual's financial responsibility for his medical services has now, more or less, become unrealistic and that other

forms of payment are necessary. The author reminds us that whatever payment method is employed, it is still the citizens who pay, directly or indirectly. What emerges from an examination of different medical systems is a need for the "managerial logic" that has been so successful in the industrial world, in order to get the maximum cost-benefit ratio. But what system is best? Although Fry cannot say, he does add, "Physicians seem most satisfied in the American system and patients least satisfied Some physicians appear very unhappy under the British system, but more than 90 percent of the public on repeated inquiries say they are well satisfied. As far as an outside observer can judge, the Soviet system is acceptable equally to profession and consumers. As for qualitative indices, it is doubtful whether the form of system has made any ultimate difference to the health of Soviet, American or British citizens" (p. 231). This is a broad generalization, and it is typical of the tone of the book.

This book is useful as much for its glaring deficiencies as for its general approach. It points out not only the usefulness of the comparative viewpoint and its limitations, but also how much further we have to go before we can truly apply the method for useful ends, given the wide differences among nations and their medical needs and resources.

Central Asia. Edited by Gavin Hambly. New York: Delacorte Press, 1970. Pp. xii+388. $9.95.

Elizabeth E. Bacon

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In *Central Asia* we have for the first time a work tracing the history of all central Asia from Scythian times to the present. Despite the book's wide range in time and space and the number of contributors, the editor, Gavin Hambly, has succeeded in achieving continuity. Following an introduction by Hambly, David Bivar traces central Asian history from Scythian times to the beginning of the twelfth century. Because writing came late to the interior of the continent, early history must be viewed from the periphery: Bivar observes the scene from the Iranian Plateau, using archaeological, linguistic, and numismatic data as well as written documents to trace the movement of peoples out of the steppes into the ken of history. After a chapter on early Tibet, Hambly follows Mongol history from Chingiz Khan to the late Mongol kingdoms of the Golden Horde and the Chaghataian Khanate. Chantal Lemercier-Quelquejay follows with a brilliant chapter on the Kazakhs. The history of the western oases from the time of Timur to the twentieth century is traced in chapters by Mahin Hajianpur and Hambly. Alexandre Bennigsen's chapter on "The Turks under Tsarist and Soviet Rule" seems somewhat tangential by including Turks outside central Asia and discussing the Basmachi (mentioned several times in the book but never identified) and an anti-Russian revolt that persisted longest among the Iranian Tajiks. Richard

Pierce covers the czarist Russian period in his usual masterly way, and Hélène Carrère d'Encausse follows with the Bolshevik revolution and Soviet policy. We are then led eastward, where Hambly discusses Lamaist Buddhism and power politics in Tibet and Mongolia. Finally, Alastair Lamb brings us up to date on events in Sinkiang under the Manchu Dynasty of China and after. The book is equipped with a bibliography, forty-four handsome photographic plates, fourteen maps, and an index.

Central Asia is an important work. For anyone wishing to identify some of the tribes that came boiling out of central Asia to make their mark on history, Bivar's chapters are most satisfactory. He has drawn on the latest research findings, and when there is controversy over interpretation, he is careful to indicate this. Many of the other contributors also provide historical supports for social and cultural interpretations

Hambly himself is weak in this respect, notably in the introduction, where he attempts to characterize central Asia geographically and culturally. In trying to contrast east and west, he makes a generalization from which the largest population group in the east—the Uighurs of Sinkiang—is excepted (p. 4); to me the obvious comparison would be between southern oases and northern grasslands. His phrase "recognizable 'oasis-mentality' characterized by an absence of intellectual curiosity" (p. 10) would cause raised eyebrows even if the reader were unaware that this oasis mentality had produced such great scientists and scholars as Avicenna (980-1037). Indeed, oasis culture is nowhere adequately characterized. The subject is poked at from different directions, most effectively by Mahin Hajianpur, but Hamly's description of oasis settlements as "comparatively self-sufficient" (p. 10) would be valid only in comparison with an Indian Village. To me the traditional oasis culture shows a high degree of specialization and economic interdependence

In his discussion of pastoral nomadism, Hambly takes the controversial view that all nomads are dependent on sedentary neighbors for "such necessities as grain and metalware" (p. 11) and accepts (p. 12) an equally controversial theory about aristocratic leadership, though he admits that it puzzles him (p. 87). Other points to which I would take exception are: that nomadic women were less chaste because they were less bound by the strictures of Islam (p. 14); that "nomadic life was generally monotonous and allowed little scope for artistic expression" (p. 15); that "creative potentialities were restricted to . . . such activities as the weaving of carpets and rugs" (p. 15). This last is questionable, since felt, not wool, is the characteristic fabric of the central Asian nomad.

Some minor criticisms: it would have been most helpful to the reader if new names and special terms had been identified the first time they appeared. For example, Chingiz Khan is described as "accompanied by his best commanders" (p. 93); the names of these commanders are introduced later, one or two at a time, often without identification. In chapter 8 there is no hint of the ethnic composition of the Golden Horde; Hambly's reference to "Mongols" applies only to the leadership. For-

tunately, Lemercier-Quelquejay indicates in her chapter the extent to which Turks and Mongols had become intermixed.

The Lysenko Affair. By David Joravsky. Cambridge, Mass.: Harvard University Press, 1970. Pp. xiii+459. $13.95.

Bernard H. Gustin

University of Chicago

This scholarly and well-written book presents a detailed analytical account of the vicissitudes of the science of plant biology, especially in its relations to the practice of agriculture, in Russia from the late 1920s to 1965. It will undoubtedly (and deservedly) become, along with Conway Zirkle's *Death of a Science in Russia* (Philadelphia, 1949) and Z. A. Medvedev's *The Rise and Fall of T. D. Lysenko* (New York, 1969), a standard source both for Soviet specialists and for sociologists of science; hopefully, however, it will be appreciated outside these limited constituencies as well, for it provides enthralling reading on one of the most bizarre episodes of twentieth-century intellectual life.

The events of the Lysenko affair are nothing short of remarkable: in a modern state, and one where science allegedly plays an especially important role in the determination of policy, science was not only suppressed, it was replaced by quackery. As the international community of science and learning watched helplessly, the Stalinist terror swept away a host of gifted scientists, most of whom, as Joravsky details, were not active anti-Lysenkoites but merely men who tried to avoid taking a stand in support of "agrobiology." At the end of the last major spasm of the terror, in 1940, even N. I. Vavilov, chief organizer and first president of the Lenin Academy of Agricultural Sciences and a scientist whose international reputation is attested to by the invitation he received to serve as president of the 1939 Edinburgh International Congress of Genetics, was arrested and sent to Siberia, where he died in prison in 1942. And yet, despite the utter failure of agrobiology to produce the miracles of increased output it had promised, it was not until the dismantling of the terror by Stalin's successors and the partial normalization of Russian scientific life in the later 1950s and early 1960s that the "resurgence of genuine biology" became possible.

The heart of Joravsky's book consists of two complementary analyses: chapters 2–6 provide a chronological treatment; chapters 7–9 discuss fundamental issues central to the episode as a whole. Of the latter, chapter 7, which details the academic conflict between Lysenkoism and biology, is especially useful in providing the technically untrained reader with a careful view of the key substantive issues. By contrast, the short introductory and concluding chapters on ideology are awkward and far beneath the caliber of the rest of the book and seem especially inappropriate in

light of Joravsky's thesis that "farming was the basic problem, not theoretical ideology" (p. ix).

Perhaps this book's chief virtue is the skill with which the author dispels prevailing myths about the Lysenko affair and demonstrates the inadequacy of attempts to account for it by pointing to ideology, a systematic pattern of terror, or "evil men." Instead, he shows that Lysenko's promotional ability, his success in appealing to populist sentiment, and the willingness of politicians to put faith in his utopian promises during a period of intense scarcity were the chief sources of his victory over "Weismanism-Morganism."

On Intellectuals. Edited by Philip Rieff. Garden City, N.Y.. Doubleday & Co., 1969. Pp. x+347. $6.95.

Anatol Rapoport

University of Toronto

In Part I ("Theoretical Studies") the concept of "the intellectual" is explored as a social role. In Part II, concrete instances are selected as illustrations of interactions between intellectuals and their social environment.

In the first two essays of Part I, intellectuals are presented as people preoccupied with a certain class of ideas. Talcott Parsons ("The Intellectual: A Social Category") relates these ideas to the "cultural system" (interacting meanings of symbols) as distinguished from the "social system" (interacting units, both individuals and groups) Accordingly, the intellectual appears as a person "who . . is in his principal role capacity expected to put cultural considerations above social" (p. 4). This role contrasts with that of, say, a ruler, an executive, or a housewife, "whose responsibilities are centered primarily in the functions of some social system as such" (p. 4).

I believe that Edward Shils ("The Intellectuals and the Powers") says something of the same sort when he singles out as the preoccupation of the intellectual "an immediate contact with the ultimate principles implicit in beliefs and standards" (p. 25). This preoccupation requires "an unusual sensitivity to the sacred, an uncommon reflectiveness about the nature of the universe, and to the rules which govern . . . society" (p. 25-26). In this preoccupation, the ideational roots of art, religion, literature philosophy, and science can, of course, be readily recognized.

The continued existence and activity of intellectuals, Shils goes on to say, are maintained not by their personal needs alone (for contact with the ultimate and sacred) but also by the needs of the society as a whole. There is, for example, a need for contact with the past and the needs of rulers to strengthen or rationalize their claims to legitimacy. The emergence of the intellectual's social role is traced to the meeting of those social needs and personal affinities of some people for the activities required to fulfill them.

From this "functionalist" definition, one might infer that the principal role of the intellectual is to verbalize and to transmit to succeeding generations the conceptual frameworks and the dominant values of a given society, at variance with the frequently observed opposite role, that of a critic of prevailing values and beliefs and of a carrier of new ideas that may eventually disrupt established social structures. Shils shows, however, that both of these roles, the conservative and the innovating, at times revolutionary, are consequences of the intellectual's specific commitments and activities. These lead invariably to a tension between the intellectual and the value orientation actually embodied in the existing institutions

On the other hand, J. P. Nettle ("Ideas, Intellectuals, and Structures of Dissent") argues that the concept of "the intellectual" must be defined apart from institutionalized forms or types of persons He examines ideas in relation to one another and only then relates them to institutional location. The result is an attempt at both a history and a taxonomy of ideas in seventy pages. The essay will not say much to someone who who is not already thoroughly familiar with the content and terminology of an enormous verbal output. Fleeting allusions to Saint Jerome and the "early Lukasc," to Gurvich and Mannheim, to the Athenians, the *philosophes*, the ideologues, and the Hegelians may trigger appropriate associations in a reader who shares Nettle's erudition. I think, however, that a book on intellectuals should have a broader audience in mind. The task of tracing the interaction of ideas is eminently worthwhile and for this reason should be addressed to readers who cannot be assumed to have detailed and specialized knowledge in this field.

The fourth paper in Part I, by Ralf Dahrendorf ("The Social Function of the 'Fool' in the Twentieth Century"), points to the role of the intellectual in modern society as that of the court jester whose duty is "to doubt everything that is obvious, to make relative all authority, to ask all those questions that no one else dares to ask" (p. 51). It is an interesting aside but constitutes hardly more than an ironic comment on serious, if not altogether successful, attempts to come to grips with an important social concept and its implications.

To relate the "case studies" to the "theoretical studies" so as to illustrate points made by Parsons and Nettle, the selections would have to range over two millennia. Instead, they range only over a century and a quarter and are confined to Western sources. Yet Part II seems diffuse, perhaps because some of the essays are "primary sources"—comments of prominent intellectuals on society and its problems—while others are "secondary sources"—comments by intellectuals on other intellectuals The editor's reasons for selecting the "case studies" are stated in the preface. Not all of them are convincing. Referring to "The Life and Opinions of Moses Hess," by Sir Isaiah Berlin, Rieff writes: "No book on Western intellectuals can be instructive without catching the strangest

case of all, that model of the modern intellectual, the intellectual Jew in the act of committing identity suicide" (p. vii). I fail to see why.

The inclusion of other essays is more clearly justified. "Barbarism is the First Danger," by Horace Bushnell, is a pre-Civil War sermon against our "bowie knife civilization" of the frontier, the precursor, as the editor states in the preface, of the modern American cult of violence. Auguste Comte's "Plan of the Scientific Operations Necessary for Reorganizing Society" is offered as an indictment of the presumptive heirs of Comte (the modern positivist social scientists), who, instead of concerning themselves with the crisis of modern society, "have become its walking symptoms" (p. viii). Also directly bearing on our own day are "English Intellectuals and Politics in the 1930's," by Stuart Samuels, and the editor's contribution, "The Case of Dr. Oppenheimer."

The liveliest essay is "Project Camelot: An Autopsy," by Robert A. Nisbet. The fiasco, related with the malicious relish it deserves, is presented as an example of the degradation of the intellectual's position by the lure of Big Time. Unfortunately, this study does not connect with any of the theoretical formulations in Part I. Intellectual prostitution is not one of the roles assigned to the intellectual by the functionalists. (Not even the court jester fulfills this role) Nor was this ludicrous affair a consequence of a dialectic clash of ideas. Finally, there is little evidence that the actors were characterized by particularly intense attachment to ultimate values, unusual sensitivity to the sacred, or an uncommon reflectiveness about the nature of the universe, which in Shils's formulation singles out the intellectual But maybe Shils and Nisbet were writing about entirely different people or social roles If so, how can these so different conceptions of "the intellectual" be related to one another? The next author or editor of a book on intellectuals ought to give this question some thought.

Criticism and the Growth of Knowledge By Imre Lakatos and Alan Musgrave. Cambridge: Cambridge University Press, 1970 Pp. viii+282. $11.50 (cloth); $3.45 (paper).

Jonathan R. Cole

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It has been almost ten years since the first publication of Thomas Kuhn's *The Structure of Scientific Revolutions* Kuhn's short book, really only an opening statement by one of a "new breed" of historians of scientific development, was pregnant with meaning and potential. Read widely by students in a variety of disciplines, it has stirred up heated debate and controversy among philosophers, historians, and sociologists of science. The core concept of "normal science," "paradigm," and "scientific revolutions," which rested on a relativistic and transitory image of "scientific truth," elicited strong criticisms from many quarters. None were louder

than those from the philosophers of science of the Popperian school. The battle continues. *Criticism and the Growth of Knowledge* is the by-product of a colloquium in the philosophy of science held in 1965, which focused directly on Kuhn's work. It consists of nine essays, seven of which are critical appraisals of Kuhn's contribution and two written by Kuhn, one as an opening statement and the other as a final rejoinder to his critics. The book contains essays by, among others, John Watkins, Stephen Toulmin, Imre Lakatos, Paul Feyerabend, and Sir Karl Popper himself.

If you like an old-style, hard-hitting British-type debate in which there are few holds barred and which deals with a "resolution" of some importance, then you should enjoy this book. For it is full of very intelligent parries and ripostes on the problems, indeed the existence, of "paradigms" and "normal science." Mixed in are assorted epithets and polite invectives that one rarely sees in published scholarly debates. Thus, for example, Margaret Masterman, a defender of Kuhn's view of how science works, at one point sardonically notes: "For the one thing working scientists are not going to do is to change their ways of thinking, in doing science, *ex more philosophico,* because they have Popper and Feyerabend pontificating at them like eighteenth-century divines; particularly as both Popper and Feyerabend normally pontificate at even more than eighteenth-century length" (p. 60).

Much of the book focuses on Kuhn's concept of normal science. Popper apparently is of two minds about the notion. Clearly, in his own work Sir Karl has dealt predominantly with the cataclysmic periods of change in the history of science and has focused on the problems of falsifying existing scientific theories. Popper never quite accedes to the existence of normal science, except perhaps in the special sense that Kuhn uses the concept. But of one thing Sir Karl is sure: if normal science does exist, it is god-awfully dull. Somehow, the idea of normal science offends Sir Karl's sensibilities. For Popper, normal science "is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it—if it becomes fashionable by a kind of bandwagon effect. . . . In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for" (p. 52).

Kuhn's position is that, "if there are revolutions, then there must be normal science" (p. 249). Indeed, the accumulation of anomalous findings which cannot be explained by the existing scientific paradigm is a consequence, albeit an unanticipated one, of normal science and a necessary condition for revolutions. Actually, within the structure of Kuhn's argument lies the implicit idea that most of the great men of contemporary science, the Nobel Prize winners and National Academy members, are "normal scientists" in the longer perspective of the history of science

Kuhn's critics attack him for embracing the methodology and perspec-

tives of the sociologist and psychologist as the means to understanding how science advances. Kuhn states: "Already it should be clear that the explanation must, in the final analysis, be psychological or sociological It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced" (p. 21). Sir Karl succinctly sums up the position of the opposition: "to me the idea of turning for enlightenment concerning the aims of science, and its possible progress, to sociology or to psychology . . . is surprising and disappointing. . . . In fact, compared with physics, sociology and psychology are riddled with fashions, and with uncontrolled dogmas. The suggestion that we can find anything here like 'objective, pure description' is clearly mistaken. Besides, how can the regress to these often spurious sciences help us in this particular difficulty?" (p 58). Throughout the book there is uniform resistance among the philosophers to the methodology of the social sciences.

As a sociologist of science, I am troubled by one point when I read this book. Too much intellectual energy of first-rank minds is being spent on debating questions such as, Does normal science exist? Many of the questions that are discussed in this monograph are clearly empirical questions. But, instead of encouraging and outlining intelligent research to find out whether the Popperians are right or wrong in their criticisms of Kuhn, the philosophical volleyball game seems to run on interminably, with the ball being thrust back and forth from one man's court to the other without any apparent end of the game in sight. Points are scored, but there are no winners. And if I might take issue with Sir Karl and his followers, I would wager that, even with the crude methodologies of the social sciences, the social scientist will in the end come up with more definitive answers about the actual operation of science and its social community than will finely tuned philosophical debate.

It is time, then, that we stepped back from the particulars of Kuhn's brilliant book and put it in perspective It is a book that raises scores of sociological questions, many of which are tantalizingly submerged just below the surface. For example, the problem of how scientific communities achieve a high level of consensus as to acceptable theoretical frameworks or "paradigms" is a basic sociological problem of some significance. The job of the sociologist of science is to mine some of the deposits in Kuhn's book. In many ways, the book calls for a new orientation toward the analysis of scientific development. It is a challenge to historians and social scientists of science to work together, while maintaining their respective angles of vision, and to pose important questions about the institutional structures of science that can be tested with empirical research. That is the real business at hand; that is what students of science should be spending their energies on. And if this be the work of normal science, then so be it.

Social Scientists and International Affairs: A Case for a Sociology of Social Science. Edited by Elisabeth T. Crawford and Albert D. Biderman. New York: John Wiley & Sons, 1969. Pp. xv+333. $11.95.

Trusteeship and the Management of Foundations. By Donald R. Young and Wilbert E. Moore. New York: Russell Sage Foundation, 1969. Pp. viii+158. $5.00.

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Two significant theoretical issues justify the combination of these books in one review: the idea of the world as a global society rather than as a collectivity of individual nation-states; and the problematic, if not critical, relationship among different institutional spheres within society, national or international.

Social Scientists and International Affairs addresses itself to the problem of the separation and interpenetration of science and policy in terms of a "sociology of social science." Although "policy" is intended to refer to the more generic phenomenon of "values," its most frequent and concrete referent in the book is U.S. foreign policy. The book consists of twenty-three papers written mainly during the 1960s but reaching back to World War II. All but two of the papers had been published previously. The contributors are all social scientists, seven of whom are sociologists, five political scientists, four psychologists, four research administrators (e.g., RAND and Russell Sage), and three with a general social science orientation.

The book is organized into four sections, the first of which consists of an integrative introductory essay by the editors and two general papers on the role of knowledge in social policy by Znaniecki and Shils; Znaniecki's contribution is a reprint from the first section of his *Social Role of the Man of Knowledge* and thus keynotes the editors' psychologistic approach to the sociology of knowledge. Shils, in the tradition of the enlightened, liberal social scientist, looks in sober fashion at the "functions" of social science knowledge for policy making and attempts to specify the "roles" which mediate between the two spheres. Shils sees that "truth is always useful to those who exercise power," but it does not occur to him that social science knowledge might not be truth but, rather, ideological justification and that, even though the "powers that be" might buy an inferior product, it nevertheless serves them well to vindicate the status quo. Shils's contribution is characteristic of the position of many of the volume's contributors. He displays a basic attitude of trust and commitment to the values of science and rationality as instruments to discover knowledge and truth and a corresponding arrogance and condescendence toward "mere opinion," the ideologies of practical politics, and the "corruption" of pure and basic research by applied and practical concerns. There is something here of the idealistic distinction between truth (reality, "being") and opinion (appearance, "becoming").

The second major section of the book is organized around the theme of the social organization of policy-oriented social science Two papers merit special attention. One is by Robert A. Packenham on "Political-Development Doctrines in the American Foreign Aid Program," which demonstrates how social science knowledge is adapted for specific policy purposes in the foreign aid program. The author frankly acknowledges the uses of social research for advancing U.S. foreign policy. It shows how deeply certain types of social science knowledge can be politicized.

Another interesting paper is by Paul de Forest on "The Social Sciences in the Foreign Policy Subsystem of Congress," published here for the first time. It illustrates the nonuse of social science knowledge by Congress and confirms the notion that social scientists frequently become "ideologues," that is, intellectuals who serve to justify and legitimize established policies instead of providing genuinely "new and different kinds of policy alternatives." In contrast to the generally descriptive-analytical approach of these papers, those in the third section, "The Affairs of Social Scientists with the Affairs of State," tend to have a more polemical, hortatory tone. This section also contains the only other original paper—an attempt by Lloyd A. Free to determine whether there is something like a world public opinion or a worldwide consensus on certain issues. While Free seeks to uncover worldwide power and influence structures underlying different, cross-cutting aspirations, values, and allegiances, he stops short of a direct identification and analysis of movements, shifts in the class structure of different countries, and contacts across national boundaries among broad protest groups such as students, blacks, and wildcat strikers Typically, the very idea of the nation-state as a distinct, unitary social system stands in the way of a more historically informed view of the world as a global society

The book's last section deals with "The Functions of Policy-oriented Social Science," in which "engineering and intelligence models" (Hauser, Lasswell) are contrasted with "enlightenment models" (Hammond, Millikan). The book concludes with an excellent forty-page bibliographic appendix.

One of the most disappointing shortcomings of the volume is its failure to deal with the realities of the international order, in short, with world society as the proper subject of a world sociology. While the title suggests such a concern, hardly any of the contributors or the editors have any vision of a "global sociology" as suggested, for example, by Wilbert Moore. Nor do the editors see the implications of their subject matter for a macrosociology; rather, they remain at the level of a sociology of social science in which a narcissistic concern with one's professional identity barely scratches the surface of metatheory, let alone a *critical* metatheory. The issue here is an old one, namely, the relation of theories of society to their sociohistorical contexts and origins. Since World War II, and possibly even since the 1917 Russian Revolution and the 1929 depression, the world has attained an increasingly visible character as a unitary, albeit

not integrated, society. Its special characteristics are that it is historically unique and that it includes subunits such as nation-states, blocks of nations, and international organizational units. The dynamics and problems of modern economic and political imperialism are hardly touched in this book; for many contributors, U.S. involvement in broad-scale political stagnation and economic underdevelopment in third-world countries is not even taken as problematic.

Trusteeship and the Management of Foundations raises the question of private wealth versus public interest as parties to the effort of bringing knowledge to bear on social problems, planning, and action. The book represents revealing reflections, by two elder statesmen of the profession, on the state of applied sociology in general and on the tension between social criticism and foundation-sponsored research in particular.

Moore's paper on "Trusteeship: A Combination of Institutional Principles" is clearly the more scholarly effort. The institutional principles in trusteeship contain certain normative elements of feudal social relations. (1) actions and responsibilities typically based on trust, (2) status honor safeguarded by class peers (not unlike the institutionalization of lay judgment in trial by a jury of peers), and (3) such affective and value-rational elements as loyalty, personal integrity, and extracontractual commitment. Moore skillfully describes the complexity of the legal relations involved, but he does not cut through to the real nitty-gritty of foundation financing and administration. After all, foundations are not just a lofty part of the superstructure of legal relations, but they are very much a part of the base—indeed, are the foundation, materially and symbolically—for a significant segment of social science research.

Young's paper on "Trustees and Foundation Management" is potentially of great interest to the organization theorist and occasionally provides unusual insights into the inner workings of trustee selection, the nature of supervision and of sanctions, the art of grant making, and, most interestingly, the question of what Young calls the "propriety in foundation programs," that is, the nature and choice of programs, the problem of ideological control, and the social "utility" of foundations. However, while the descriptive part of Young's paper is too general and not comparative enough to be of great value for organizational analysis, the more interesting and touchy questions of the foundations' ideology are discussed in an almost defensive, apologetic tone. *Qui s'excuse s'accuse?*

The treatment of lay control in this little book is fascinating but also frustrating. A few knowledgeable hints notwithstanding, the authors do not come to grips with the basic problem of the hardly visible but continued operation of an economically dominated *civil* society, even though there is visible progress in the separation of church and state, in the franchise, and in the granting of various *political* and *legal* rights Lay control is class control. The deep misgivings over "maximum feasible participation" by the poor on the boards of private welfare agencies have their counterpart in the jealously guarded procedures for selecting the

right "type" of trustee for the foundation board. Underlying the supposed virtues of lay control is the assumption of impartiality, the separation of economic from political interest, or of religious and other ideological interest from the affairs of science. But "civil liberty," within the framework of the modern capitalist or even postcapitalist (welfare) state, describes a condition which still guarantees the dominance of economic interests without intervention by the polity, regulatory powers, or legal guarantees. The issue is assumed to be one of political emancipation and, generally, the separation and "functional differentiation" of institutional spheres from one another. But it is precisely one of the consequences of political emancipation to hide and even protect the continued operation of economic inequality. The fiduciary principle, that is, trust, as the institutional core of modern foundations is a peculiar anachronism; but as a basis for "distributive justice," the trust relationship becomes a cornerstone in social exchange theory. Thus, it may be said without bitterness that trust is also one of the preconditions for exploitation. It remains, therefore, problematic to what extent the foundations do, in fact, act on behalf of the public interest and the common good (as they are entrusted to do) or to what extent they act on behalf of certain elite and class interests, as well as the status quo.

Being an Anthropologist: Fieldwork in Eleven Cultures. Edited by George D. Spindler. New York: Holt, Rinehart & Winston, 1970. Pp. xv+304. $4 95.

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If we consider "fieldwork" as an activity closely related to science, then anthropological fieldwork has had a rather strange history. Two identifiable periods have passed, and period 3 is just beginning In period 1, the rubber-data stage, anyone—serious scholar or traveling layman—could collect data on the exotic customs of the noncivilized "primitives." The key phrase which describes this period is perhaps "I was there, therefore I know." Those who "were there" wrote what they knew in poetic and often moralistic language The data they gave us were weak and often so vague and general, in basic areas of social life, that a variety of interpretations were possible. Differently put, the data were stretchable, hence the name "rubber-data stage."

With the coming of the great "masters" of anthropological thought and method—Franz Boas in America and A. R. Radcliffe-Brown and B. Malinowski in Europe—fieldwork climbed out of the rubber-data stage and into the holy-data stage. Fieldwork now was done by university-trained professionals. Yet "professionals" is hardly the right word for them, for they defined their work as being far more than a profession. Anthropology was a discipline that could save man from himself. A well-

developed "science of man" could save the world! These early "masters" and their students saw anthropology as a calling, demanding a life of sacrifice and dedication. These idealistic people therefore considered anthropology as (almost) a religion. Therefore, the data collected—the output of anthropological work—were holy. And more to the point, the way good data were collected was somewhat like the way many important things occurred in religion: in mysterious ways. Somehow, those with the correct ideals and purity of soul, those whose motives were good—such people were able to achieve rapport with the natives. Somehow such rapport led to "good data." And somehow such information was properly analyzed and synthesized into its proper vehicle of communication: the ethnography. Vision information is rarely subjected to tough "truth tests"; few questions are asked concerning reliability and validity. Similarly, the data collected during the holy-data stage in anthropology were rarely criticized for the possibilities of mistake and misrepresentation.

For more than a decade now, anthropological publications and less formal discussions have signaled the imminent demise of the holy-data stage. Such discussions also heralded the birth of a new stage: an exciting period that I call "fieldwork as science." Fieldwork as science has suddenly hit anthropology, and some of us are having trouble handling what Merton might call the unforeseen consequences of the marriage between anthropology and science. One secret but real concern is that we may lose some of our tribal identity: we worry that it will become increasingly difficult to tell us apart from our sibling rivals, the sociologists.

The "in thing" to do in anthropology today is to write books on method. Understandably, more and more anthropologists have jumped on the bandwagon created by fieldwork as science. Following the Goffmanic *Behind Many Masks* by G. Berreman (Ithaca, N.Y.: Society for Applied Anthropology, mono. 4, 1962) is the very human *Return to Laughter* by E. Smith (Garden City, N.Y.: Anchor Books, 1964), the gentle *Stranger and Friend* by H. Powdermaker (New York: W. W. Norton Co., 1966), the revealing *Diary in the Strict Sense of the Word* by B. Malinowski (New York: Harcourt, Brace & World, 1967), and the tell-it-like-it-is *Marginal Natives* by M. Freilich (New York: Harper & Row, 1970) comes a welcome addition: *Being an Anthropologist: Fieldwork in Eleven Cultures*. This volume, edited by George Spindler, describes the research of a number of scholars, many of whom have international reputations in anthropology: Homer Barnett, Alan R. Beals, Jeremy F. Boissevain, Robert K. Dentan, Norma Diamond, C. W. M. Hart, John T. Hitchcock, John A. Hostetler and Gertrude E. Huntington, R. Lincoln Keiser, Edward Norbeck, and George and Louise Spindler.

In this very readable book, scholars and researchers present themselves as humans with a tough problem to solve. In brief, their problem and challenge is to understand a culture completely different from their own. The reader who carefully studies *Being an Anthropologist* will almost get an "I was there" feeling. He will also come to appreciate the strains

and stresses of anthropological fieldwork—an occupation with more than its fair share of intellectual excitement and physical discomfort. In *Being an Anthropologist*, eleven marginal natives tell their story—and they tell it well.

Arcology: The City in the Image of Man. By Paolo Soleri. Cambridge, Mass.: M.I.T. Press, 1969. Pp. 122. $23.00.

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Despite the obscurity of Soleri's language, the technological and ecological advantages of arcology are quite simple and straightforward: architecture built so that it does not destroy nature. Instead of sprawling suburbs, an arcology is a compact three-dimensional structure When built on a city scale, its *density frees surrounding* countryside to host natural ecological cycles, thereby restoring the earth, the air, and plant and animal life to an unpolluted state. Like the medieval city, the arcology allows the citizen to walk beyond its limits to the country; thus, man would become both a city and country dweller Internally, the arcology (city) is free from pollution because cars are not necessary. Industry, housed in the inner core of these giant structures, produces by-products such as heat and wind, which in specific arcologies, Soleri proposes, would heat the remainder of the environment, thereby eliminating wasteful duplication of energy production.

Much of the technology for the thirty arcologies in this book does not yet exist, so it cannot be criticized Yet certain problems are clear from the outset. For instance, once an arcology is built it cannot be expanded for population increases. If cities can be planned with an optimum population in mind, the restriction may be advantageous in that overcrowding would be eliminated. But would overflow populations simply build another arcology? Or would zero population growth be legislated? Another example is that Soleri finds free enterprise and laissez faire philosophy wasteful and contemptible but does not give a clue as to the type of collective alternative he would find valuable or workable.

The fact of change itself cannot be a criticism of arcology. The appropriate question is whether the changes are worth the costs, material and psychic. Through his consistent failure to explore the ramifications of the physical forms for social and individual life, Soleri makes it difficult for the citizen to evaluate the worthiness of his proposals. One defense might be that he is describing concepts, not plans. In this case, however, the social prerequisites for and consequences of these arrangements are as important as the concept itself.

Another effort to skirt the issue is his discussion of "equity" and "congruence." Equity is specifically human and stems from man's capacity to

do wrong. Congruence is almost Nature herself, the weaving of disparate forces into constant coordination. He argues that if human society is not congruent with nature, then talk of equity is shortsighted. At several points in the thirty-one physically floppy and unwieldy pages of essay, he simply declares that social problems will be solved. For example: "The car will follow the horse to the pastures of sport and eccentricity," and "War hardware will destroy us or will be destroyed by us" (p. 31), and, most baldly of all, "If there is no mention of segregated minorities, of slum clearance, of exploiter and exploited, of tax unfairness, of bossism, of children killed by delivery trucks, of skid-row peripatetics, of pets not allowed, of profit incentives, of self-help, it is because one assumes that in time the skill of man will take care of them all. The foundation of equity is thus granted" (p. 1).

He proclaims that the individual living in an arcology will experience joy, awe, wonder, delight, curiosity, willfulness, growth, continuity, wholeness, fullness, etc. This proposition is presumably meant to counter the criticism that the individual will feel insignificant in such a vast and complex structure. Soleri declares that on the structural level the arcology can only inspire while, on the microenvironmental level, people will be able to build and install their own homes and objects within the spaces alloted to them.

He spends a surprisingly large portion of his essay justifying the three-dimensionality of his cities. He repeatedly extolls the virtues of vectorality: it is life itself, in that life is not random but has tendencies (vectors). Vertical vectorality is especially good because it is like man himself, an upright cluster of energy and emotion—thus, the subtitle of the book: *City in the Image of Man.* In contrast, Soleri likens today's cities to basically two-dimensional plant life.

In asking students to evaluate the latent function of Soleri's arcologies for individual and group life, I frequently found the terms "beehive" and "anthill." The physical arrangement of our living and working spaces is an emotionally charged one; thus, Soleri may be putting his energies to their best use by elevating description to that of poetic vision with positive images intended to compete with current practices. He tackles the beehive charge directly: an arcology is composed of more than the superbrain (coordinating computer) and human brains. Humans have minds which are capable of reflection and choice, unlike ants and bees, whose brains operate by instinct. (Stating the crucial differences of choice, he then abandons the issue of good versus bad choices, parenthetically "discarding the intervention of evil" [p. 25]. In an equally offhand manner he assumes that greed and the "misplaced exhilaration" of exercising power must eventually give way to rationality and humanism.) An arcology is also different from a beehive because of privacy. He claims that leisure will soon come to all, and when it does it will free the individual from concern with maintenance activities so that he is left with time

for his own thoughts and fantasies. Soleri claims that this freedom, instead of owning one's own property, is real privacy.

Another justification for compactness and three-dimensionality evokes evolutionary principles. Soleri says that with each evolutionary jump an organism becomes internally more complex. To do more in the same space requires "miniaturization." A three-dimensional city minimizes the distance between any two given points and thereby conforms to the evolutionary principle of miniaturization. Soleri stands apart from some social critics in calling for more rather than less internal differentiation in social organization.

Soleri's writing is most charitably viewed as consciousness raising with regard to the relationship between the natural and the man-made environment. To be taken seriously, however, his own consciousness about the relationship between form and behavior will have to be raised.

The Suburban Myth. By Scott Donaldson New York: Columbia University Press, 1969. Pp. 267. $10 00.

Edith Kurzweil

New School for Social Research

The Suburban Myth, by Scott Donaldson, a professor of English and history and former journalist, consists of an admirably inclusive review of general and journalistic articles and novels as well as sociological studies about suburbs, for the most part after World War II. The author defines suburbs as communities surrounding metropolitan areas and usually dependent upon the latter. Since in the literature suburbs have been blamed for many of the ills of our society, the author sets out to destroy what he sees as the popular "myth" that life in the suburbs is confining, conforming, and complacent and finds evidence to prove that this life can be satisfying, rewarding, and, by implication, "relevant." He traces the persistence of the Jeffersonian myth, the Americans' worship of pastoral life, life on the farm or at least close to nature, where participatory democracy existed and, above all, where a true spirit of community based on dependence and reliance upon neighbors was frequently needed for survival. Americans have not given up this myth and are trying to create, in the suburbs, a midway point between city and rural life. Particularly since World War II, suburbs have mushroomed around large cities, replacing the pastoral landscape suburbanites were seeking. These suburbs were frequently planned in poor taste in order to provide the builders with a substantial profit and to provide "instant" housing for the returning veterans and their expanding families. The author agrees with Dobriner that "the suburban zone is becoming increasingly heterogeneous in economic functions and in class, ethnic and racial characteristics" (p. 53). He finds that middle-class Americans buy the type of house they can afford and move when they can afford a better house into new or estab-

lished but expanding communities. This leads to the existence of suburbs which can be homogeneous or heterogeneous in terms of age, occupation, income, and ethnicity. The author's extensive knowledge of the literature as well as his fluent style are used well in juxtaposing opposing points of view, and in my judgment, he argues correctly that suburbs cannot be blamed for the ills of our society.

In the 1950s, after Americans had weathered a long and deep depression and had successfully fought a war, it seemed imperative to create financial and emotional stability, to recreate what was thought of as the American community ideal as well as to satisfy a deferred gratification for comfort and ease. In American sociology this was expressed in the proliferation of studies within the framework of functionalist theory, with its emphasis on tendencies toward equilibrium. No wonder then that Whyte's *Organization Man* was found to be conforming not only at work but in his "neighborly" suburb as well. The author finds that "the trend toward conformity and objections to that trend stretch back to the beginnings of American history" (p. 106). Tocqueville, though a great admirer of American democracy, was fearful that Americans who worshiped both freedom and equality would give up the former for the latter. Josiah Royce, as far back as 1902, was troubled by the standardization of modern industrial civilization. Donaldson says: "The intellectuals see individualism submerged by the waves of the industrial and technological revolutions which in standardizing products have supposedly standardized ways of life and thought." I wonder if this trend can be dismissed so lightly just because it existed throughout history? Or is this trend more apparent now with increasing technology, automation, mass communication, and programming—not only of products, but of men and women? Can people in the suburbs any more than people in the cities escape the tendencies to standardize life-styles and thought along with the standardization resulting from centralized control by government and businesses? Donaldson is correct in saying that the suburbs are not to blame for the ills of our society, but this does not eliminate these ills. Although suburbs have frequently been used as examples of pseudocommunities, of overconformity—the myth the author seeks to expose—there, as well as elsewhere, the spirit of community (of *Gemeinschaft*) which accompanied Jefferson's ideal agrarian society of individualists is generally lacking. It can emerge in times of crisis, as it did when the citizens of Bloomington, Minnesota, banded together to get rid of polluted water; it can be revived around issues of political and civic improvement, such as better sanitation or streetlights. Alain Touraine, who is pessimistic about the future of *Post-Industrial Society,* sees possible improvement in the actions of individuals, in their opposition as consumers of standardized goods and thoughts, and in actions similar to the one in Bloomington. He probably would agree with part of Donaldson's conclusion that "we cannot go home again to our pastoral dreamhouse" but would disagree with him that "the real house in the suburbs may be better." Only when suburbanites (and

urbanites) become aware and learn to choose freely between conformity and individualism, when they make conscious choices and face the problems of the society at large, will the deplorable myth be destroyed; only then will "the real house in the suburbs be better."

It is interesting to observe how someone outside the field of sociology reviews and evaluates the literature If he does not use sociological method, he avoids imposing his own ideology on particular data in a non-criticial way. Yet, he does have a thesis, and it is this which must be questioned when applied to suburbia rather than to modern society as a whole.

The Industrial Urban Community: Historical and Comparative Perspectives. By Nels Anderson. New York: Appleton-Century-Crofts, Inc., 1971. Pp xii+438. $10.95.

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University of North Carolina

This volume is a revision of the author's *The Urban Community A World Perspective* (New York, 1959). No mention of this fact is to be found in the book. Although there are minor changes in chapter titles and numerous references to recently published materials have been added, the new version of the book does not differ much from the old The strengths and weaknesses of the original edition, as well as the format, are rather faithfully reproduced in the present volume.

The author describes his approach as comprising five "themes." These are identified as (1) population and urbanism, (2) pollution and sanitation, (3) our civilization's global arena, (4) technology, sciences, and cities, and (5) the social pertinence of work The selection and presentation of these "themes" offers an early clue to the unsystematic character of the work. Indeed, the treatment that follows is chatty, rambling, unabashedly evaluative, and cursory.

A recitation of the content of the book would be tedious, for it touches upon the array of topics that has become conventional in urban-sociology texts. It includes some definitions, a glance into history, a bit of ecology, a brief discussion of population trends, some remarks on mobility and residence changes, and then a series of chapters on problems, classes, the family, work, leisure, welfare, social control, and planning. The material in each chapter is organized under a series of intriguing subheads which, unfortunately, promise more than is delivered. Important theoretical issues and provocative empirical materials are passed over lightly with hardly a probing question or a critical comparison. The historical and comparative perspectives announced in the subtitle of the book consist in a great many rather casual references to the works of historical and foreign scholars. No effort is made to present those data in ways that would permit an inspection of the similarities and differences in urbaniza-

tion as between past and present and developed and developing societies. It is a pity that the opportunity to explore cross-historical and cross-cultural comparisons was not exploited. Few students of urban phenomena are better equipped than the author with foreign experience and wide-ranging knowledge of the literature. The rich bibliography cited in the footnotes is perhaps the most valuable contribution of the book.

In sum, this book grossly underestimates the sophistication of the present-day undergraduate. What he needs is not simple descriptions of urban conditions and problems interlaced with an author's gratuitous opinions, but a theory that might aid his understanding and a methodology that will enable him to investigate urban phenomena. The book is lacking on both counts.

Urbanization in Nigeria. By Akin L. Mabogunje. London: University of London Press, 1968; New York: Africana Publishing Corporation, 1969. Pp. 353. £2.50; $8.50.

Dan R. Aronson

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This is an important book for all of us. Professor Mabogunje introduces comparative theory and sophisticated methodology, and hence *order,* into the study of Nigeria's cities, thus both setting a pattern for analysis of other west African data and refining general concepts. He examines the degree to which urbanization in Nigeria reflects the same forces that have led to urban growth and development in other parts of the world, thereby demystifying the still frequent presentation of west African urbanism as a unique and exotic phenomenon. He gives perhaps the best summary of historical materials on the older cities of northern and western Nigeria (I found the section on the primary urbanization of the north especially exciting), yet stresses principles and concepts of economy, location, and specialization, thus building a social scientific perspective into social and political history. Finally, he does his social analysis as a committed citizen, thus "relevantly" relating scholarly abstraction to policy critique and recommendation.

The strength of any book should be its challenge and debate over the issues it raises. This will certainly be a good starting point, but certain inconsistencies of Mabogunje's argument must receive more attention if we are to develop the discussion with clarity.

For instance, "no apology is offered" for what the author agrees is "an economic interpretation of urbanization" (p. 5). But on the one hand, his account of the origins of urbanization in general and of the emergence of nineteenth-century Sokoto in particular makes "the role of power groups . . . fundamental not only for the genesis of urbanization but also for its direction and progress once generated" (pp. 38–39). On the other, the factor analysis of thirty-two variables from the one sound Nigerian

census (1952; that of 1963 remains politically clouded) yields seven factors, accounting for 84.3 percent of the total variance. Only the first, "urban economic function" (accounting for 21.5 percent of variance) is clearly economic. Factors 3 and 5 are demographic, while the remaining four (accounting for 38.8 percent of variance) are in fundamental ways *cultural* (the "regional," "Ibo," "female urban employment" ["largely culturally determined" (p. 166)], and "minority" factors). If the historical and statistical materials reveal significant cultural and political dimensions of the urbanization process, then the "economic interpretation" needs rethinking in the light of the very data upon which it is based. The resulting theory might, incidentally, be of more help in assessing the changes wrought in Nigeria's urban places by the recent civil war, since those changes—the depopulation of certain urban areas, the relocation of functions, the "decentralizing" of some cities—were only indirectly "economic" in causation.

A second set of inconsistencies relates to the structure of the book itself. Sections derived from the author's doctoral dissertation on Lagos, his extensive and well-known material on Ibadan, and his more recent involvement with computer methods often seem juxtaposed rather than integrated. Thus, data which indicate to us whether Ibadan, "the apogee of pre-European urbanization," or Lagos, the prime "metropolitan creation of the colonial era" (p. 27), typifies the cities generated by his stages of urbanization would allow more confidence in the omission of any northern or eastern town for detailed analysis Again, the near shorthand with which the factor analysis is presented in chapter 6 is unfortunate when the author considers the chapter "the most important in the book" (p. 5). The perseverance he advises should be matched by the care in argumentation he takes in other parts. Also, the concepts of hinterland and central place, and the factors seen as crucial for understanding Nigerian urban patterns in general, are not referred to in the discussion of the two cities in the second half of the volume, where the theoretical orientation switches to internal structure in the tradition of Burgess, Hoyt, Proudfoot, and others. Limited areas of Nigeria and single cities need to be examined in detail in the light of Mabogunje's earlier attentions; descriptions of internal structure are necessary but not provocative. This is not to say that concepts that are useful at the macrolevel must be relevant as well at the microlevel, but only that the data presented do not allow the reader to judge whether Lagos and Ibadan truly represent "types of urban development" in Nigeria, as Mabogunje asserts, or whether they are simply examples of Nigerian cities.

Finally, there is a disconcerting mix of ideologies which inform both policy recommendation and analytical framework. To dissect the impact of the imperialist economy, Mabogunje asks, "What happens when the new spatial economic integration focuses the flow of commodities . . . on a few selected points, with a view to facilitating the export of these commodities out of the country?" (pp. 141–42) To improve the precolonial

internal economy "would have meant that the British colonial venture was not to serve the interest of the British people but that of the Nigerians primarily" (p. 143). Of course, the British impact cannot be erased, but Mabogunje indicates no major revaluation of overall urban policy in Nigeria—of urban versus rural expenditures, of the location of transport nets, or of the best sites for industry, for example. The critical rhetoric quoted notwithstanding, is he therefore satisfied with the general trend of urbanization in Nigeria set in motion by the British? Again, he states clearly that life in the core areas of Ibadan "is particularly integrated, satisfying to the full the warmth of intimate human relations" (p. 235). Is he not falling into a now-routine Western trap when he calls central Ibadan at the same time "a sprawling blot . . . requiring vast sums of public money and imaginative planning to clear and redevelop" (p. 235)? Finally, citing the tremendous social and microeconomic disruption caused by the Lagos Slum Clearance Scheme, he still claims bald generality to the principle, now being severely challenged by citizens and planning agencies elsewhere in the world, that the "invasion of one type of land use by another is . . . a healthy sign of development which can hardly be prevented" (p. 308).

We can easily grant the needs articulated in his last chapter for regional planning, for generating commitment from migrants to their cities of increasingly longer sojourn, for a metropolitan authority for Lagos, and so on. But Mabogunje could have greatly advanced the debate on Nigerian development that will be stimulated by his book if he extended his own thinking on the tougher political issues he raises but does not always grapple with very thoroughly.

The Structure of Community Power. Edited by Michael Aiken and Paul E. Mott. New York: Random House, 1970. Pp. xi+540. $9.95.

Terry N. Clark

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This is a long and expensive collection of materials, but its general comprehensiveness and the quality of a few original contributions make it a valuable work. It includes many of the basic methodological and substantive articles that helped establish the field of community power: Floyd Hunter and Robert Dahl on their respective approaches, methodological criticism by Nelson Polsby and Raymond Wolfinger, Robert Schultz on bifurcation, methodological refinements by Charles Bonjean and Linton Freeman et al., Peter Bachrach and Morton Baratz on nondecisions, Delbert Miller on international comparisons, and much more. It is satisfying that most articles are reprinted in their entirety, including the long hard-to-locate study of C. Wright Mills and Melville Ulmer.

The extensive introductions to the sections by the editors are careful

evaluations of such topics as the concept of power, factors affecting configurations of power, methodological issues, and comparative community power studies.

And this is not just a "reader"; there are several important original contributions. On pages 443–63 is the most comprehensive published report on John Walton's quantitative analysis of case studies in sixty-one communities. As usual, he presents his findings with impressive compactness: twenty-one hypotheses are tested relating community characteristics, discipline, and method to the degree of centralization of decision making. Slow population growth, absentee ownership, and competitive parties, for example, were all found negatively related to centralization. A valuable appendix presents his complete coding scheme and the coded results for each community—a handy source for future researchers. Aiken (pp. 487–525) analyzes these and additional data and uses more careful breakdowns by type of city. Suggestive findings: Amos Hawley's MOP (manager, proprietor, official) ratio apparently measured the *opposite* of what Hawley intended; decentralized decision-making patterns lead to *higher* rather than lower outputs (for public housing, urban renewal, poverty programs, and model cities). (Similar findings emerged from our data on fifty-one cities [*American Sociological Review* (1968), pp 576–93].) Both Walton's and Aiken's findings, however, are limited by their austere use of multivariate analysis. This is in part a function of the small number of cases. At least one frequently cited result, however, changes dramatically when more cases are considered. *neither* discipline *nor* method are related to centralization of power

A recurring theme is absentee ownership A careful article by Mott (pp. 170–80) refines the bifurcation thesis, following additional fieldwork in Ypsilanti He shows how, despite noninvolvement by leading managers, other company officials become extensively involved in community affairs. The goodwill they help create becomes a very real resource for certain decisions. Another original article by Robert M. French (pp. 180–89) brings out the importance of the position of absentee-owned enterprises relative to local enterprises in another community: withdrawal of absentee enterprises is facilitated if dynamic locals intervene Walton and especially Aiken demonstrate the contribution of absentee-owned firms to more decentralized decision-making patterns across the country.

Articles by Mott on power (pp. 3–18) and factors leading to centralization (pp. 85–99) are straightforward but more elementary than the other original materials.

As the preface indicates, the volume was begun in 1959; several selections were added after that time, but certain recent developments are understandably underemphasized. This is still one of the leading compilations of materials for course use; I have used it already.

There is a good bibliography (including Ph.D. theses in the area—a pattern others should imitate). There is no index.

Crisis in Brazil. By Octavio Ianni; translated by Phyllis Evelath. New York: Columbia University Press, 1970. Pp. 244. $8.50.

Shepard Forman

University of Chicago

In *Crisis in Brazil,* the Brazilian sociologist Octavio Ianni sets out to explain the "series of internal and external political and economic ruptures" which led that nation on its path of development between World War I and the military coup d'état of 1964. The original Portuguese title translated as "The collapse of populist democracy in Brazil" (*O colapso do populismo no Brasil*) and reveals the author's principal theme of clarification of the role of the masses in the creation of a nationalist phase of Brazilian political and economic development.

In Part I, "Politics and Development," Ianni explicates a series of developmental models through which Brazil passed during the first half of this century, beginning with: (1) the export of raw materials and the import of manufactured goods until 1930; (2) the substitution of imports by which Brazil underwent rapid industrialization in the 1950s; and (3) the international associate model of political and economic interdependence on the United States after 1964. An alternative socialist model is assumed to have been a viable possibility on various occasions in those intervening years (although this assumption is not really pursued). According to Ianni, the "tensions and conflicts generated by the succession and coexistence of these developmental models" comprise the Brazilian dialectic. The modern history of Brazil is the history of the struggle between a national populist democracy led by an ineffective Left and a technocracy led by an internationalist elite, resulting in a right-wing military dictatorship with a total commitment to the United States.

In describing the export phase, Ianni eschews the "two Brazils" model of an archaic agrarian sector lagging behind a modernizing industrial sector and notes the degree of integration between the two and the way in which they articulate with international political and economic structures A conflict exists in the appropriation and formation of capital in the two sectors and in the dependency relationships which the agrarian sector has to establish abroad while the industrial sector seeks an independent and nationalist line of development.

In Part II, "The Nationalist Epoch," Ianni attempts to describe the emergence of the nationalist model of industrial development based upon a government policy of substitution of imports and characterized by a growing independence in foreign policy and the integration of the masses in a populist democracy which began in 1930 under the dictatorship of Getulio Vargas. During this phase, the ideology of nationalist development was shared by the rural and urban proletariat, middle classes, the military, and the industrial bourgeoisie, who all joined to help "strengthen the rupture with traditional society and predominant foreign sectors." The arena of decision making was shifted to Brazilian shores, and the federal

government participated fully as a creative force in the growth and diversification of the economy and in the establishment of the Labor Laws, which coordinated political and economic activity among and between workers and industrial elites. The general maneuvering for improved positions within the established social system militated against the formation of a proletariat class consciousness and interclass antagonisms. For their part, the Brazilian Left was unable to successfully invest the revolutionary capital generated by the intersection of international crises and internal conflicts. The Left engaged the Brazilian masses politically, but never gave them power. Rather, it manipulated the masses as part of its own bourgeois political strategy. Nevertheless, student and labor agitation in the form of demonstrations and strikes filled an emergent and vulnerable middle class with an overriding fear of "communism and corruption" and led to their strong support of a military coup d'état and dictatorship.

In Part III, "The New Order and Structural Dependence," Ianni analyzes the roles of the middle classes and the military in the 1964 coup d'état, the subsequent suppression of the Brazilian Left through the suspension of normal democratic processes, and the reintegration of Brazil into a system of international capitalism dominated by multinational corporations headquartered in the United States The military government replaced a nationalist ideology of development—political independence and economic autonomy—with an ideology of modernization, the domestic corollary of interdependence. Brazil is now economically, militarily, politically, and culturally (but not irrevocably) tied to the United States.

Brazilian history in the first half of the twentieth century has been tumultuous. There is no single study that I know of that describes or explains it adequately. The definitive statement on the complex processes of Brazilian political and economic development will only be reasoned from a variety of interpretations after sufficient data for making mature judgments come to light. For the moment, we must content ourselves with a variety of interpretations, of which Ianni's is one useful and provocative example. Ianni is an astute native observer who brings forth considerable documentation in support of his interpretation of political positions and events. Nevertheless, his interpretation is based on personal observation, and the evidence (comprised of "surface" data in the form of economic and social statistics and published statements that purport to reveal the actors' intentions) is presented as supporting material and not as basic data. What is really needed is a full historical record of events and a rich contextual account of their unfolding, including the ideas, ideologies, and actions of the individuals and groups who participated in them. Only then can we really expect to understand political and economic history in process. Conflict alone must not be confused with process.

It is good to note that this book was written for a Brazilian audience and that Ianni assumed a background knowledge and degree of sophistication in things Brazilian in his readership. The reader who is unfamiliar

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It is good to note that this book was written for a Brazilian audience and that Ianni assumed a background knowledge and degree of sophistication in things Brazilian in his readership. The reader who is unfamiliar

with the details of recent Brazilian political and economic history will encounter a long list of abbreviations for government agencies and political parties that have little meaning for him and many oblique references to people on the Brazilian scene who are hardly known abroad. The brief glossary and guide to abbreviations that is appended to this edition offers only limited help. It does not facilitate understanding the nature of decision making in a complex society among "those groups who exercised authority directly and indirectly."

Crisis in Brazil is hardly an introduction to the complexities of contemporary Brazil. But it is a stimulating interpretative essay on the recent political and economic history of a dependent nation.

Student Politics in Chile. By Frank Bonilla and Myron Glazer. Student Movements Past and Present series. Edited by Seymour Martin Lipset. New York: Basic Books, 1970. Pp. xiv+367. $8.50.

James Petras

Pennsylvania State University

Most social scientists, journalists, and historians writing about Latin-American students have emphasized the violent, irrational, "pathological" aspects of Latin-American university politics. The image that emerges is one in which lifetime "student" activists, moved by psychological disorders, disinterested in or even hostile to the search for knowledge, obsessed by political action for its own sake, have been instrumental in the retardation of Latin-American universities.

From two quite distinct vantage points, the empirical findings of Frank Bonilla and Myron Glazer on Chilean students refute the conclusions of the largely discursive essays which dominate the study of student politics. Based on in-depth interviews and a thorough reading of almost all available research dealing with three generations of student leadership and organization (1918–22, 1936–40, and 1956–57), Bonilla concluded that radical student leaders in Chile were not psychologically maladjusted individuals. Discussing the student leaders of 1918–22, Bonilla found that "realism and objectivity are high among these subjects on all topics discussed; they showed, in general, a well balanced perspective on the self and in groups. Externally directed aggression seemed controlled and not excessively affect-laden" (p. 73).

Glazer's study of Chilean student attitudes toward professional commitments, largely based on an interview schedule applied in 1963–64, found that "political and social ideology seemed to give strong impetus to the rejection of the remnants of traditional society and increased the desire for change and modernization. Among these students there was often a direct marriage between their political and professional goals" (p. 266).

Glazer found a high correlation between degree of professionalism and degree of political involvement, thus debunking the notion "that pro-

fessional activism is a direct impediment to professional modernization" (p. 266). Despite their virtues, however, the two essays contain several weaknesses.

They have such different focuses that it is difficult to consider this a book. There is a weak link between Bonilla's concern with student political organizational change and Glazer's preoccupation with student professional commitments.

Bonilla's discussion and generalizations regarding the earlier generation (1918–22) were based (of necessity) on a very small number of individuals (eight), which is an inadequate basis from which to theorize with any confidence. Glazer's study of student attitudes fails to include the law school; law is the single most important profession in terms of recruitment of national political leaders. Chilean political development has been dominated most of this century by the graduates of law school, which makes it difficult to understand why Glazer excluded it in favor of a marginal school such as science. Both authors fail to discuss adequately the obvious impact of U S. imperialism and Chilean cultural dependence on student political and professional attitudes. Glazer mentions an incident in which students of science, after initially agreeing to cooperate with Glazer's study, decided to withdraw, stating (among other reasons) that U.S. authorities could use the responses to exclude radical science students from receiving scholarships to study abroad—an important aspect of their professional training. Glazer fails to follow this up and to consider the possible political inhibitions and reactions engendered by U.S. control of an aspect of professional training.

The attempt to "update" the book, to take account of recent developments, is below the level of the rest of the book. The newer material in some cases conflicts with the previous discussion, and little serious effort is made to explain the discrepancies. Professors, according to Glazer, were the principal models which students looked toward in deciding their future careers. Yet in the period subsequent to his study, it was precisely the established professors who were one of the targets of a massive student reform movement. The science school described earlier as apathetic is, in the latter period, fully engaged in political activity. Moderate bureaucratized Christian Democrats described as characterizing Chilean student leadership are replaced by Marxists and in some universities (Concepción) by supporters and activists in the principal urban guerrilla group (MIR). Tired and rejected Social Democrats of the 1950s (interviewed by Bonilla) are hardly representative of the dynamic Marxist socialists of the late 1960s (now in power), intent on nationalizing major industries, banks, and commerce. The authors fail to take account of the dynamic underlying socioeconomic forces which were emerging in the late 1950s and 1960s and which are now radically reordering Chilean society. But for that, the authors would have had to elaborate an analytical framework that went beyond Parsons and Lipset and leaned heavily on the works of Marx and Lenin.

The correlation between high professional commitment and political activism that Glazer found is probably a reflection of the political tolerance of Chilean political culture and may not be generalizable in more repressive societies. In less politically mature nations such as Brazil, Argentina, and the United States, a university political activist quickly learns that his professional opportunities will be severely circumscribed if he persists in radical political action. The repressive political system hence may produce the divorce between professional and activist roles.

The essays of Bonilla and Glazer have their greatest merit in suggesting a whole series of new problems and issues that have thus far been inadequately explored by U.S. social scientists intent on demonstrating the irrationality of student activity instead of considering the political context in which student activity is inserted.

Black Music: Four Lives. By A. B. Spellman. New York: Schocken Books, 1970. Pp. 225. $1.95 (paper).

Gary Schwartz

Institute for Juvenile Research

Black Music examines the fate of four contemporary black jazz musicians. Spellman does not theorize extensively about the role of the jazz musician in our society, nor does he engage in aesthetic polemics on behalf of the jazz avant-garde: he lets the musicians speak for themselves. His book is organized around interviews with Jackie McLean, Ornette Coleman, Cecil Taylor, and the late Herbie Nichols, all of whom talk at length about the vicissitudes of their professional careers.

Taylor, Coleman, and Nichols developed improvisational techniques which violated the canons of the reigning "bebop" style in jazz. Unlike innovators such as the late John Coltrane and Charles Mingus, whose affinity for the jazz mainstream was never seriously doubted, these musicians do not have a prestigious and vocal following in the jazz community. For the most part, their work elicits indifference or hostility from its dominant coteries. Public response to their music is minimal primarily because they rarely have an opportunity to perform, and when they can perform, it is usually in a setting that denies their musical integrity. These musicians exist on the margins of the jazz world.

This does not mean that a jazz musician with the proper credentials leads a glorious and prosperous life. Jackie McLean's career provides the necessary perspective on the jazz life as a whole. As Charlie Parker's protégé (insofar as a mercurial genius like Parker can be said to have had a protégé), McLean had an enviable status as a young musician, and his association with Parker should have insured a relatively smooth career. But McLean also suffered from the many frustrations that plague a serious jazz musician's life.

In addition to the greed which blunts the sensibilities of promoters and

night club owners, there is an insidious but pervasive pressure on jazz musicians to create a slick, easily recognizable format for their music and, if it is commercially successful, to stick with it. Record companies and club owners want a proven product. The performer must bear the costs, financial and otherwise, of artistic experimentation.

This demand for a musical style that remains static and hence easily identifiable constitutes the supreme contradiction for the dedicated jazz artist whose music is predicated upon improvisational freedom. Jazz thrives on artistic risk taking; a good musician must live with the constant possibility of failure. Spellman puts it this way: "the discovery had gone out of bebop—it had become as formalistic as any movement does once it has solved its original problem. It was that indefiniteness of not knowing how the music is going to sound before it is played that enhanced its emotional expression" (p. 83).

As innovators, the musicians discussed in this book feel very strongly that the blues roots of jazz should allow each performer to search for whatever musical form is consonant with his basically emotional response to the tradition from which he derives. For these musicians, then, insistence on the timeless validity of any musical form distorts the essential nature of jazz—the rhythmic spontaneity which has its roots in African music. Consequently, they are willing to experiment with any Western musical form as long as it serves the vital impulse which underlies black modes of musical expression. This feeling about jazz is memorialized in the title of an Ellington tune, "It Don't Mean a Thing If It Ain't Got That Swing."

Ironically, the currently established jazz idiom, often referred to as "hard bop," was itself a reaction to the increasingly routinized qualities of jazz improvisation. "Bop" dissolved the sterile conventions of the preceding "swing" era. No longer limited by the harmonic simplicity and rhythmic uniformity of "swing," jazz experienced a massive burst of creative energy. The late night sessions at Minton's Playhouse in Harlem and the numerous clubs along Fifty-second Street in Manhattan were a few of the now legendary locations associated with the careers of famous musicians like Thelonious Monk and Dizzy Gillespie. At this time, there were many musicians and listeners who charged that the new music was too esoteric, bombastic, or chaotic. Spellman documents a similar reaction within the jazz community to new modes of experimentation. But the rigidity of jazz audiences and performers appears to have increased. Coleman and Taylor, in particular, do not have an intensely committed group of followers and devotees to sustain the inspiration of the "bop" revolution. Their creative efforts resonate in a social vacuum.

This compelling book can be read in many ways. What strikes me as particularly significant from a sociological viewpoint is the corrosive effects of racism on the entire jazz enterprise. The cultural life of our society rests upon a pernicious dichotomy between "high" and "popular" culture. Our society is grossly insensitive to the value of cultural forms which are

not protected by the aura of classical European "high culture." Young experimental composers and performers who adhere to this tradition have their careers subsidized by foundation grants and prestigious competitions, whereas their counterparts in the jazz world are forced to survive as part of the "entertainment industry"; that is, their work as black musicians is categorized as a transitory and artistically trivial form of amusement. As an "entertainer," the jazz musician must find a relatively large audience for his music; he must create a style that has popular appeal. This is an untenable position for a serious musician who judges the worth of his music in terms of the intellectual and emotional challenges it presents to those who hear it.

In my opinion, the false facade of the musician cum popular entertainer is partially responsible for the intensely competitive and destructive ethos of the jazz community. Many jazz musicians become obsessed with "cutting" one another. Technical facility and inventiveness become the standards these musicians use to judge their relative merits, and the result is an atomistic social universe where there is little internal support for those who would expand the musical horizons of the jazz tradition. For the musician who cannot or will not adapt to the exigencies of the "music business," there are almost no facilities for his musical development. He is condemned to a very lonely as well as impoverished life, and his one consolation is that he can remain faithful to the cultural sources that nurture his identity as a black musician.

This book reveals the circuitous but nonetheless ineluctable manner in which our society corrupts the authenticity of black culture. By imposing impossible financial and artistic constraints on those who would create cultural forms that express the emotional currents of black experience, our society isolates the jazz musician from the source of his inspiration. This is an important book, and anyone interested in the sociology of culture can gain much from reading it.

Deep Down in the Jungle: Negro Narrative Folklore from the Streets of Philadelphia. By Roger D. Abrahams. 1st rev. ed. Chicago: Aldine Publishing Co., 1970. Pp. ix+278. $8.95 (cloth); $3.95 (paper).

John F. Szwed

University of Pennsylvania

The dust jacket of this book tells us that in its first (1964) edition it was an "underground classic," a nice way of reminding us that it went totally unreviewed the first time around but had considerable influence nonetheless. Now, in a substantially revised form, we are given a second chance.

Briefly, this book is a large collection of toasts, jokes, and taunts as told by a group of young black males in Philadelphia and is accompanied by an analysis of their setting in interpersonal and familistic relations.

But perhaps the most striking thing about Abrahams's book is simply the documentation of the existence of a rich tradition of oral folktales and poetry among urban Afro-Americans. It is striking chiefly because parallel traditions were recorded for rural Afro-Americans in literally hundreds of publications between 1860 and 1940 but after that were rarely mentioned in print. Why? Was it that once Afro-American verbal lore was documented there were no changes and developments worth recording? Or was it that blacks in the cities lost the skills and traditions which had once been so widely known? Neither, it seems, for Abrahams's book is evidence to the contrary I think, rather, that the writings on race relations by members of the Chicago school of sociology (especially E. Franklin Frazier) and Gunnar Myrdal created an orthodoxy so influential that it made any discussion of the culture of lower-class Afro-Americans appear gratuitous, if not racist. That is to say, once the assumption was established that Afro-Americans were so thoroughly culturally stripped by slavery and further damaged by segregation and prejudice, it appeared that documentation of any aspect of their lives could reveal nothing more than evidence of the atrocities of racism. For social scientists, Afro-American culture was no more than (to use Ralph Ellison's phrase) the sum of its brutalization. Thus, the impetus to do ethnographic accounts of the lives of lower-class Afro-Americans was substantially reduced. But perhaps more to the point, the impact of the literature of race relations was such as to make descriptive studies of American blacks appear politically dangerous to civil rights advocates: the unstated law was (and is) that any evidence of black adaptation, ingenuity, or creativity was suspect and that any positive attention to black culture was racist in intent.

I call attention to this example of social science mythmaking (some would call it stereotyping) because it goes a long way toward explaining why books such as Abrahams's were not researched and written over the last thirty years, and, for that matter, it helps explain why this specific book was ignored eight years ago. Problems of this sort help us to understand what seems to have been happening to Abrahams himself between the two editions of his book, for in a remarkably candid introduction to the second edition, he describes his own confrontation with these questions. He suggests that, while collecting and writing up the first edition, he uncritically accepted the conventional wisdom on the pathological status and operation of the matrifocal family, or what he calls the "mammy family." In the interim, he has conceived of alternative meanings of the nature and operation of Afro-American institutions and relationships, especially since he has done fieldwork with blacks elsewhere in the United States and the West Indies. He has thus become cognizant of the ethnocentrism implicit in superimposing white middle-class *assumptions* about white middle-class behavior on top of *assumptions* about black lower-class behavior, blacks thus emerging as defective and deviant. As a consequence, in this second edition Abrahams underplays the importance of the mother as a destructive force in the black community; indeed, she even disappears from the index in the new edition!

In revising his argument, Abrahams has also clarified the central thinking in the book, a rhetorical meaning of folkloristic behavior and its influence in everyday life. At the same time, he has rid himself of the culture-bound Freudian interpretations in the first edition. What emerges is a portrait of a highly verbal, articulate people whose daily lives are charged with the importance of wit, metaphor, and subtlety in a thousand ways. And what we see, too, is an uncommonly honest attempt to wrestle the meaning from a body of—yes—*literature* quite different from that of the investigator.

All of this is not to say that I am entirely happy with Abrahams's analysis. I feel that, like most folklorists, he still leans too heavily on a limited body of cultural data and that he moves too quickly from verbal lore to social structure, particularly as we do not yet have an adequate description of the latter. Yet this book (to be read along with another of his, *Positively Black* [Englewood Cliffs, N.J., 1970]) is the first in many years which takes the leap from the sociology of pathos toward recognizing Afro-Americans as culture bearers and creators, that is, as men. And as such, *Deep Down in the Jungle* must be required reading for all who would attempt an understanding of lower-class Afro-American life. Hopefully, the book, underground no more, will become simply a classic.

Who Needs the Negro? By Sidney M. Willhelm. Cambridge, Mass.: Schenkman Publishing Co., 1970. Pp. xvi+266. $4.95 (paper).

Alphonso Pinkney

University of Chicago and Hunter College

Since the black rebellions of the 1960s and the advent of Afro-American studies, hardly a day passes without the publication of at least one book about the inhabitants of America's internal colony. The quantity of books in this area is such that it is impossible to read all of them. And it is probably just as well, because many of them contribute little to a better understanding of the plight of American blacks.

Who Needs the Negro? does not fall within this category; Sidney Willhelm has written an important book in which he predicts a grim future for America's blacks. He sees the black rebellions of the 1960s as a result of the transformation from industrialization to automation in the mid-1950s. This shift has rendered the black population irrelevant to the American economy, for machines have diminished the need for the black colony's chief export to the larger society: cheap labor. Since white Americans no longer need the labor of blacks, Willhelm feels that genocide might be in store for blacks. The response of blacks to automation has been to rebel against a society which has cast them aside and in the process stripped them of feelings of self-worth.

The author dismisses family instability and urbanization as sources of black poverty, because both of these blame blacks for problems created

by white racist society. He argues that, for blacks, wars have generally been periods of economic improvement because of manpower needs, but once the fighting has ceased, whites resort to discrimination as usual. Finally, he attacks the theory of relative deprivation as an explanation for the widespread black protests of the 1960s on the grounds that this proposition is ahistorical.

Willhelm then presents extensive data on discrimination against and exploitation of blacks in the labor force, on black unemployment and underemployment, and on the effects of automation in rendering blacks economically worthless. He concludes that "white America can, for the first time, easily bear the economic costs for implementing its racial values to the point of excluding the Negro race." The combination of technological change and racism foreshadows the obliteration of black people. In its relations with blacks, American society has moved "from exploitation to extermination."

Although he points out that his prognosis does not represent "ultimate realities," Willhelm suggests that, if blacks continue to express their despair through property destruction, they will suffer the same fate as did the Indian population. Black communities today are roughly analogous to Indian reservations, and black rebellions are the equivalent of Indian uprisings. Hence, just as Indians were slaughtered for nearly three centuries, genocide remains a possibility for blacks.

Willhelm presents a mass of historical and contemporary data to support his thesis. However, he is sometimes guilty of exaggeration, error, and distortion. For example, he uses such phrases as "several social scientists," "these authors" (p. 30), and "a number of white writers" (p. 186); yet he cites only one person in each case. He reports that in 1967 only seventy-one out of 2,700 students at Columbia University were black, when he must have had reference to Columbia College. He compares the enrollment of black students in universities in various sections of the country and concludes that integration at the University of Mississippi, with 1 percent blacks, exceeded that at the University of Wisconsin, with 0.9 percent. What we are not told, however, is that at the time, the proportion of blacks in the state of Mississippi was 43 percent, while in Wisconsin it was 1.9 percent. Finally, his frequent use of the term "colored" to refer to blacks is confusing, because one sometimes gets the impression that he is including members of other racial minorities, only to learn later that he is referring specifically to blacks.

These criticisms are not meant to minimize the significance of Willhelm's impressive book. One may not agree with his prognosis, but given America's penchant for the use of force to solve problems (real or imagined), coupled with the addiction of its citizens to racism, his prediction could become the ultimate reality for blacks. In addition to the internment of the Indians, the Japanese were incarcerated during World War II, and some two decades later hundreds of thousands of South Vietnamese people are being put in concentration camps which are called "strategic

hamlets." Although Willhelm may ultimately prove to be correct, black people have demonstrated an amazing capacity to survive the brutalities of white racist society. Nevertheless, when black radicals declare that the black struggle is essentially one of survival, who can deny that they are correct?

Teaching Standard English in the Inner City. Edited by Ralph W. Fasold and Roger W. Shuy. Washington, D.C.: Center for Applied Linguistics, 1970. Pp. xvi+141. $5.00 (paper).

Doris R. Entwisle

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This volume is one of a number coming out of the Center for Applied Linguistics. It is authored collectively by sociolinguists, who use linguistic analysis in a social-psychological frame of reference. They have broad interests—the effects of language use on upward mobility, what features of a dialect lead to social stigma, social stratification as correlated with speech characteristics, and the like. Sociolinguistics of this genre, recent upon the American scene, is hard to overrate in importance, since language and language use are fundamental topics of inquiry in studying any social system.

The essays in this volume are all written from a biloquialist perspective: namely, that in dealing with dialect differences, the most desirable approach is to teach standard English to speakers of nonstandard dialect *without* denigrating or trying to discourage speaking of the nonstandard dialect under appropriate conditions. The child learns to switch from one dialect to another, depending upon the occasion or situation. He may speak one dialect on the playground and another in the classroom. (Fully bilingual programs, where middle-class speakers of standard English are taught a nonstandard dialect, have not yet been tried, although some see them as the culmination of a biloquialist approach. They might be especially robust from a psychological standpoint, since speakers of nonstandard would see their more advantaged classmates struggling to learn nonstandard.)

Ralph Fasold and Walter Wolfram (pp. 41–86) present in nontechnical language the information currently available on the linguistic features of Negro dialect. They discuss in detail phonological differences, the absence of final consonants in Negro dialect, for example, and grammatical differences, the omission of forms of "have," for example. The usages they recognize and recommend are not necessarily the traditional ones. They suggest that the teacher should use his own casual speech as a standard. The rich detail they give for Negro nonstandard alerts a teacher to those aspects of dialect that can most easily lead to misunderstanding and confusion between standard and nonstandard.

Irwin Feigenbaum (pp. 87–104) uses knowledge of standard and non-

standard to suggest a set of classroom drills or exercises. He recommends the display of similar forms in standard and nonstandard—"He work hard" versus "He works hard"—so the students can sort out and identify the features of the two dialects likely to be the most confusing. He also recommends discrimination drills, identification drills, translation drills, and response drills, and suggests how to use such drills in the classroom.

Wolfram (pp. 105–19) addresses the problem of the pedagogical sequencing of standard English. Those features of black English that are most stigmatized should be worked upon first (the distributive "be"; for instance, "He don't be busy all the time"). A table of black English features is given that suggests an order of cruciality—the order in which the features of Negro dialect should be eliminated.

Teachers until recently received little training, formally or otherwise, in linguistics and often have seen dialect as "error." Thus, instead of seeing a dialect as an orderly and consistent linguistic system, they have denigrated speech forms other than standard English and have seen their mission as stamping out such speech. What teachers need to know about language is a topic that Roger Shuy has addressed himself to repeatedly. Here (pp. 120–41) he points out that summer workshops and other crash programs cannot really do the job. The college-level language arts program needs overhaul, he believes, with the inclusion of material in social dialects and fieldwork in the language of children. William Stewart's essay (pp. 1–19) comments on the suitability of foreign-language teaching methods for "quasi-foreign" languages and the need for more precise knowledge of dialects so that teachers can put such methods to work. Joan Baratz (pp. 20–40) calls for urban-language specialists to teach children who speak Negro nonstandard One feels that all these recommendations deserve thought, perhaps more thought than their authors have given them. It is one thing to point to deficiencies in procedures and attitudes and to make some casual and sweeping pronouncements. It is quite another to engage in the tough evaluations that such reforms require, to start to see how much variance in school performance can be accounted for by problems of dialect per se, unconfounded with social status, economic deficit, physical disability, or IQ.

The issues that inhere in a bidialectal approach to education are more pervasive and more severe than this book suggests. It treats only the Negro urban nonstandard dialect, but even this dialect, if such a thing can be labeled, varies considerably from one city to another or within a single city. To take account of dialect across the country, at least seven major dialects must be dealt with (see R. L. Venesky, *Non-Standard Language and Reading*, Working Paper no. 43 [Madison: Wisconsin Research and Development Center for Cognitive Learning, 1970]). This is no easy matter; the dialects have not yet been completely identified. But technical problems aside, which language should be taught, and how, cannot be isolated from all the social and political problems that surround

us. Children apparently find it easy to learn any language when it is seen as relevant to their day-to-day needs and goals.

Crime, Police and Race Relations: A Study in Birmingham. By John R. Lambert assisted by Robert F. Jenkinson, with a foreword by Terence Morris. Oxford: Oxford University Press. 1970. Pp. xxviii+308. $9.75.

M. Chatterton

University of Manchester

Is there more crime in colored immigrant areas, and are immigrants disproportionately engaged in it? What are the implications of the changing ethnic composition of these areas for police training and practice? Are the children of immigrants more likely to become delinquent than their white peers? These were the questions addressed in the research reported in this text.

Concentrating on one area of Birmingham, John Lambert presents a profile of crime and disorder within three zones and eleven areas of that police division. The data are derived from police statistics covering a four-month period and include 2,046 reported indictable crimes, 528 events leading to arrests for nonindictable offenses, and 500 prosecuted motoring offenses. Somewhat unusual, but nonetheless relevant to a discussion of police discretion and police-immigrant relationships, is the inclusion of 562 "minor incidents," mainly disputes, which the police handled without invoking legal sanction. Persons arrested during the period provide information on offender characteristics, namely, residence, nationality, etc.

One of the merits of this study is that it not only identifies the areas of crime, disorder, and criminal residence, but examines in considerable detail the substantive nature of the offenses. Information, for instance, about the types of premises entered, the value and nature of goods taken, and the relationships between parties to a dispute or assault provide more data on the context of these events and present a more realistic picture of the state of reported crime in each area.

Taken in conjunction with census data on the area, these statistics show that crime was unevenly distributed throughout the division and that immigrant areas had a higher rate of crime and disorder. However, the material on detected offenders reveals that, although much crime was local in character, immigrant groups, with the exception of the Irish, were not as involved in it as their neighbors.

Crime and disorder are aspects of the pathology of those areas in which immigrants are forced to live, Lambert concludes.

The crime survey also proves that reported crime was typically petty thievery and unlikely to be detected. Furthermore, the clear-up rate was markedly affected by a group of offenders who had committed additional offenses that were taken into consideration. Considered in this light, the

prevailing emphasis on the "crime-control" function of the police appears somewhat anomalous.

Lambert develops this point in the following section and discusses those aspects of policing which were undervalued in the study area and the consequences of police attitudes and practices for both police-immigrant relations and race relations in general.

By comparison with the previous section, the chapters on the police are disappointing. Lambert raises again many of the now familiar issues in police work and organization but without the systematic approach of the earlier chapters. He does not make sufficient use of his experiences "on the beat" to support his argument and to analyze the operations of the agency which produced the statistics. His points about the marginal position of the policemen in the area, for example, their attitudes toward and beliefs about the stereotyped outgroup, would carry more weight if fieldwork data had been incorporated into the argument at this stage. The "minor incidents" used in the statistical study, which did not result in an arrest and which involved colored people, would have been particularly relevant here. One is left in doubt as to whether Lambert's repeated suggestion that the police should be more sensitive and responsive to the needs of immigrant communities implies that the handling of such incidents militated against the establishment of a "sense of well-being and confidence" on the part of the colored citizens involved.

Lambert's general point that police training should educate policemen about the situation of the immigrant is basically sound. Police administrators and instructors, however, will look for more positive guidance about what "good" police work, in Lambert's terms, means in relation to the incidents the patrolman has to deal with and perhaps with reference to the events Lambert observed firsthand.

In the final section, the author explains why, on theoretical grounds, one would expect to find different rates of delinquency among children of different immigrant groups. He develops his hypotheses by using the case files of agencies involved with "families in need" and delinquents.

The use of material from these case histories is the least satisfying aspect of these last chapters. It has been shown elsewhere that social workers tend to construct their case files around the manifest fact of the subject's delinquency and organize their observations and reports according to accepted theories of delinquency causation. Consequently, it is difficult to assess how useful this kind of information is for Lambert's purposes.

Any single project having limited research resources that explores the types of issues chosen in this study must necessarily concentrate on certain aspects and be content to raise others for more detailed future research. This study is no exception, and as Lambert says, the research was intended to be exploratory and, consequently, raised more questions than it answered.

America Adopts the Automobile, 1895–1910. By James J. Flink. Cambridge, Mass.: M.I.T Press, 1970. Pp. xii+343. $12.50.

Neil Harris

University of Chicago

Few machines have influenced our society as pervasively as the automobile. Almost all aspects of our economic life, our leisure activities, our systems of law enforcement, and our daily living patterns have been affected by the motor car. Its importance cannot be exaggerated. Yet, until now, no historian has examined seriously either the variety or the character of these effects. With this volume, James J. Flink, a historian at the University of California, has tried to fill the gap. Examining the first fifteen years of the automobile's adoption, Flink presents more than the traditional subjects of technical improvement and economic organization. He has mined the era's magazines to portray the expectations of contemporaries and the solutions they worked out to meet the challenge of the automobile. Licensing techniques, road building, automobile clubs, garaging, and patterns of distribution are among the subjects he surveys

Despite the fact that Europeans dominated early automobile technology, Americans quickly claimed the vehicle as their own. The first American automobiles were manufactured in the mid-nineties; by 1912 the country contained almost 1 million motor vehicles and was producing 378,000 annually. Despite poor roads and an absence of governmental interest, Americans quickly picked up the motoring habit. Climate, terrain, population density, highway construction—all these affected the pattern of purchases less than income distribution. As soon as they could afford to, even before, Americans invested in cars. The phenomenon even affected the real estate market, home mortgages being taken out to finance the purchase of autos.

Why Americans fell in love with the automobile is a fascinating inquiry Obviously, its speed and relative efficiency were appealing, and long arguments demonstrated its greater economy over horse-powered vehicles. But, as Flink demonstrates, more cosmic hopes played about the new invention, as they often do in the morning days of innovations. Publicists excitedly spoke of ending the dangerous contrasts between city and country by lessening the isolation of farmers and aiding the commuting of urbanites. Perhaps the surge to city living might be over. Cleaner air, cleaner streets, and an end to traffic jams were also some promises of the future. They were promises made but unkept, of course, but true believers fervently accepted them. The obvious advantages the car offered physicians, hospitals, and fire and police departments were also marks in its favor, though governmental agencies on all levels were slow to realize the possibilities.

Flink's book does a good job of cataloging the enthusiasms and surveying the responses. But because he lacks a coherent and articulated model of innovation, Flink has lost his chance to make a significant overall

argument. The details overwhelm the larger picture. The bibliography contains few theoretical studies, mentioning only Everett Roger's *Diffusion of Innovations*. It also is low on standard works in European automotive history. Flink concentrates on popular magazines, which yield useful insights but produce them casually and spasmodically. At moments in the book, for example, Flink refers to differences between Europe and America in the reception of the automobile, differences reflecting Europe's larger, more centralized bureaucracies and its smaller clienteles. But a more systematic comparison would have given shape to the argument. "Governmental certification of the competence of automobile operators was well established in Europe by the turn of the century," Flink tells us, while the practice was nonexistent in the United States. The greater caution and more elaborate controls in Europe reflect not only governmental practices, but a whole range of social attitudes and expectations, and can be related to European treatment of railroads, telephones, and telegraphs. Obviously, Flink feels the cultural context is important, but without a sustained argument or a coherent model, it is difficult to learn its true significance. Content analysis can carry the researcher only so far. Perhaps the next volume, which the author has promised us, may follow up more systematically the roads not taken in this one.

American Baseball. Vol. 1: *From a Gentleman's Sport to the Commissioner System.* Vol. 2: *From the Commissioners to Continental Expansion.* By David Quentin Voigt. Norman: University of Oklahoma Press, 1966 and 1970. Pp. xxxi+336; xxii+350. $7.95 each.

Stephen F. Barsky

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Only recently have we begun to see systematic research on sports among sociologists. There are only a handful of men in this field: J. Coleman, J. Dumazedier, H. Edwards, G. Kenyon, J. Loy, J. Phillips, D. Riesman, W. Schafer, M. B. Scott, S. Stone, and a few others have published research. UNESCO and the International Sociological Association have published the *International Review of Sport Sociology* since 1966.

There is room for sociocultural histories and sociologies of team sports. While *American Baseball* might seem to fit here, it does not. These two volumes contain a rather episodic history of baseball, seen largely from available documentary sources, unpublished records, and a few interviews. Baseball, as portrayed in this work, evolved from street and field games played by children and gentlemen amateurs into commercialized entertainment with sporting overtones; sociological interpretation is nearly absent.

David Quentin Voigt, of the Department of Sociology and Anthropology at Albright College, divides the evolutionary history of baseball into "eras." The "Gentlemen's Era" begins at about 1845 with the evolution

of a game which would be recognizable today, passes through a stage of commercialization and the founding of the National League, and closes during the late 1870s with the appearance of professional baseball journalists and a specialized jargon. The "Golden Age" spans a dozen years ending in 1892. During this period, commercialized baseball became firmly established. This establishment was the result of three elements: a profit boom, the development of drawing-card "heroes," and the strengthening of the National League as an outgrowth of an abortive players' revolt and other conflicts.

The "Feudal Age" begins with the second National Agreement, which produced a trustlike organization of all major-league clubs in late 1891. During this period, the club owners consolidated their control over the game, the players became "proletarianized," racism became obvious, and the last significant rule changes were made. The nature of the National Agreement encouraged public attacks on owners as "magnates" and monopolists. The owners, feeling as well a pinch at the ticket office, encouraged the formation of a second major league from a top minor circuit. This was the American League, formed in 1902, which was expected to return baseball to the profits of the eighties through competition and to mollify public opinion. Volume 1 ends here.

Volume 2 opens with a recapitulation of this evolution and continues with what Voigt calls the "Silver Age," from the turn of the century to 1920. During this period, the major leagues fought off a renegade third league, baseball generally suffered through the First World War, and gambling and fixing of games received some publicity. The "Black Sox scandal" of the 1919 World Series demonstrated the need for tighter control of the game, and in 1920 Judge K. M. Landis was hired as the first strong commissioner. The initiation of a strong commissioner system begins the "Second Golden Age," which ends just after World War II. During this period new heroes were created, the use of a livelier cork-centered ball became universal, which produced a strategy change from finesse to power, and profits soared.

The end of the war introduced what Voigt calls the "Plastic Age," marked by racial integration of the majors, the decline of the minor leagues, and increasing competition from professional and college football, basketball, and ice hockey for television money and spectator attention.

This two-volume work is impressive in promise. It contains much information drawn from baseball newspapers and magazines, popular magazines, and records, and represents lots of archival digging. My objections to it are mainly focused on volume 2. While volume 1 attempts some analysis, the second half of *American Baseball* is unsystematic and almost oblivious to the deep changes in baseball stemming from changes in the society of which it is a part. Baseball's audience today is an aging cohort of fans, and its practitioners are an increasingly black and Spanish-speaking group of athletes. Voigt does not deal with these issues, except

to mention "prejudiced fans" on page 298 of volume 2 when he discusses the introduction of black players. He does not mention prejudice in relation to other players, coaches, managers, or umpires.

Other sociological problems are also given only brief mention. On page 64 of volume 2 Voigt devotes a short paragraph to the social origins of Silver Age players. On page 71 he mentions a "1915 study of the backgrounds of 146 'crack' players by a criminologist" but does not give a citation. On page 93 of volume 2 Voigt mentions James T. Farrell as an Irish-American Chicago White Sox fan, but he does not talk about the North Side (Cubs) versus South Side (White Sox) rivalry in Chicago. Voigt speaks of urbanization and the decline of the minor leagues on page 147 of volume 2 but does not expand upon the changing demography and ecology of baseball except to mention changing population distributions in a picture caption on page 272. On pages 206–7 of volume 2, the author introduces the idea of the social origins of fans, but then drops the topic. There is no discussion at all of such important topics as the trade-union-style militancy of players and umpires and the changing style of players' presentation of self.

Another distressing feature of this work is the number of misinterpretations, apparent misprints, and other errors in the data. I checked the data in volume 2 in the new *Baseball Encyclopedia* (New York, 1969), which Voigt also cites. I found many more errors than are reported here; the following are examples.

In trying to make the point that, in 1915, Athletics owner Connie Mack was stingy, Voigt claims that another owner purchased one of Mack's "stars" plus two "star" players Mack had "snubbed" (p. 21) These players were, respectively, Jack Barry, Babe Ruth, and Ernie Shore. However, in no case could any of these players be called stars by 1915: Barry was mediocre, Ruth had appeared in only four games in 1914 (his first season), and Shore was ranked third or fourth pitcher on a six-man staff. Again, in trying to say that Ty Cobb "batted poorly" in 1906 after a family tragedy (p. 54), Voigt tries too hard. In fact, 1906 was Cobb's second year in the league and he batted .313, up more than seventy points from 1905. On page 283, Voigt claims that professional soccer is currently a threat to the attention baseball receives. In fact, professional soccer has died several deaths in the United States, although it is extremely popular elsewhere in the world.

Other kinds of errors appear frequently in volume 2. On page 34 the photograph is obviously reversed (the Chicago "C's" are backward, and the men appear left-handed). In reporting the 1923 World Series, Voigt says that the New York Giants "won the first two games. Twice beaten and shut out once, the Yankees came back strongly. Ruth blasted three homers . . . and the Yankees drubbed the Giants four straight times" (p. 173). In fact, the Giants won the first and third games, and two of Ruth's homers came during the Yanks' victorious second game. The Yankees won the last three games in a row. These errors are not vital to

the argument of the work, but they impair Voigt's believability. There are also annoying misspellings: Powell Crosby for Powell Crosley (p. 244), "military" for "militant" (p. 266), and Schultenreich for Schultehenrich and Pizzola for Pezzolo (p 219). Again, these are not important but they are annoying.

In sum, *American Baseball* is a well-written informal history of the sport, based largely on analysis of documents spanning more than 100 years but with little sociological insight and only fair accuracy. As an informal history, it is a worthy contribution to knowledge and a good basis on which to begin some more systematic research.

Sport and American Society: Selected Readings. Edited by George H. Sage. Reading, Mass.: Addison-Wesley Publishing Co., 1970. Pp. x+422. $4.95 (paper).

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In spite of the fact that sociologists apparently spend as much of their leisure time participating in, watching, and talking about sports as does the rest of the American population, they have devoted surprisingly little of their working time to this area. What little research is done is almost all limited to studying either athletics in the schools or the career patterns of professional athletes. Passing mention of sports is sometimes made in courses on the sociology of leisure, the sociology of occupations, the sociology of education, or American society, but to my knowledge the only courses devoted to the sociology of sports are found in physical education or recreation departments, where the emphasis is far from that needed for the development of a sociological understanding of this area. If there were a good book in this field that a course could be built around, or even one that would be useful as supplementary reading, students might be encouraged to look into this area, and in a few years the flow of significant sociological research on sports might increase. With this in mind, it was with great hope that I picked up *Sport and American Society: Selected Readings*, edited by George H. Sage. I then found myself wading through eighty-five pages before finding anything that could possibly be considered sociological. After that things improved, but not as much as I would have liked.

On the plus side, the volume is well-organized and pulls together materials which might otherwise be missed by sociologists. The thirty-two articles are grouped into six sections: "The Heritage of Sport in America," "Sport and the School," "Sport and Social Status," "Race and Sport," "Women and Sport," and "Sport and Society." One article ("Participation on Interscholastic Athletics and College Expectations," by Richard A. Rehberg and Walter E. Schafer) is reprinted from this *Journal*. It, along with the articles by James S. Coleman ("Athletics in

High School") and Gregory P. Stone ("Some Meanings of American Sport") and possibly those by Johan Huizinga ("Play Contest as Civilizing Functions") and Roger Caillois ("Toward a Sociology Derived from Games") will be familiar to many readers of this *Journal*. Most of the others will not.

As with most large anthologies, the contents vary greatly in both quality and intended audience. Some articles require that the reader be able to interpret MMPI scores, evaluate critical ratios, compare standard deviations, or understand the results of an analysis of variance. Other articles come from the pages of *Today's Health* or *Sports Illustrated* and bear titles like "Sis-Boom-Bah! for Amalgamated Sponge." Also in common with most large readers, the editor fails to relate the various articles or explain contradictions in their findings. For example, in one place we read that students with high athletic achievement have slightly higher IQs than those with low athletic achievement (p. 148), while another study reports that "intelligence was significantly lower for all athletic groups than for the nonathletic group" (p. 206). In addition to the usual problems faced by anthologies of this sort, Sage has taken on another one· his book, the back cover tells us, is "designed for courses in Physical Education, Psychology, and Sociology " As even the most mediocre archer could tell him, it is difficult for one arrow to strike three separate targets, even if it does have thirty-two barbs.

As I indicated in my opening lines, there is a need for texts in this area, and so I would not be so harsh on this one if it were all that were available. But it is not. I suggest that anyone considering adoption of *Sport and American Society* also examine *Sport, Culture, and Society: A Reader on the Sociology of Sport*, edited by John W. Loy, Jr., and Gerald S. Kenyon (New York, 1969). While sharing all the faults outlined above, it has them in lesser degree and, most important, it is more consistently sociological. Unfortunately, the comparison is not all one-sided: *Sport and American Society* is a paperback selling for $4 95 and thus can be assigned as supplementary reading, while *Sport, Culture, and Society* is hardbound and sells for $8 95. Surprisingly, while many of the same authors appear in both volumes, there is no actual duplication of articles. Thus, another possibility would be to assign both books, perhaps deleting the less relevant articles from the required reading. Certainly any sociologist interested in the sociology of sports should own both volumes.

Social Stress. Edited by Sol Levine and Norman A. Scotch. Chicago: Aldine Publishing Co., 1970. Pp. vii+292. $9.75.

Social and Psychological Factors in Stress. Edited by Joseph E. McGrath. New York: Holt, Rinehart & Winston, 1970. Pp. xi+352. $9.50.

David L. Dodge

University of Notre Dame

It would be an insurmountable task in the space provided to attempt to

review these two volumes in detail. Twenty-six authors from a variety of disciplines, with varying perspectives, approaches, and areas of concern, richly delineate in twenty-eight chapters the many perplexing issues, multiple conceptual models, and problems of research and definition apparently inherent in the study of human stress. As should be expected, at times they diverge greatly on certain items, but more often than not this is due to "field specifics" of their research problems or stances. They often concur and are mutually supportive of one another. What is of particular benefit and encouragement for the reader, and which becomes quite apparent throughout each volume, is that the field of "stress study" has direction and that its content, as represented by the many works in these volumes, is being guided by a broad, generic architectural frame of reference. The thoroughness of each volume underscores fully the increasing concern in sociology and social psychology with the study of stress and, concomitantly, a greater appreciation of its complexity.

A large amount of theoretical and research activity has been going on for some time in several disciplines concerning a wide range of phenomena which could readily fall under the rubric of "stress studies." However, the productivity and mutual assistance that might have accrued from integrating or relating the many findings and interpretations of these studies have not come about, because the activities have tended to be largely "field specific"; the subjects studied are heart disease, hypertension, suicide, deviant behavior, etc., or the approach has been "discipline specific" (e.g., medicine, social psychology, sociology, anthropology, etc.) and has utilized a variety of concepts as referents of stress or referents productive of stress such as conflict, alienation, frustration, aversive events, anxiety, etc. What has sorely been needed has been an anthology of this work which would give students of stress a view of who is doing what, what the current state of affairs is, and where we are going from here. These volumes provide the service. Each accomplishes an ample portion of its goals, providing a broad view of the field and the promise it holds for future research and theory building. They also complement each other. Whereas Sol Levine and Norman Scotch's volume will probably appeal much more to sociologists and anthropologists and their emphasis is more on where we currently stand, George McGrath's volume appears to be more oriented to the social psychologist and the direction research is taking. There are, of course, excellent articles on the other goals in each volume, but these two goals represent the general thrust of each volume. Consequently, the student of stress would be well rewarded to have both volumes on his reference shelves.

What is stress? How is it defined and manifested? Is stress accumulative? Is its magnitude enhanced by multiple simultaneous exposures to stress stimuli? Do cognition and then appraisal of stress stimuli determine the degree of stress experienced, or vice versa? Are stress typologies fruitful? What conceptual model of stress is most fruitful, or is it possible

that a particular model is more applicable to one research endeavor than to others? These and myriad other questions and issues are wrestled with in each volume.

Despite their decision as editors not to confine the contributing authors to any standardized format, content structure, or definition of stress, Levine and Scotch have come up with a tight and pithy book. Their volume is divided into four sections containing discussions of the sources, nature, and consequences of stress. Their concluding chapter does much to tie the individual papers together, revealing persistent and uniform themes which designate "where we are at" and point out future work needed. Robert Scotch and Alan Howard's paper, "Models of Stress," includes a model of stress that rises out of a careful scrutiny of the strength and limitations of eight basic competing models evident in current literature. Indeed, their model and their critical review of the eight other models of stress provide the reader with a superb background for appraising and integrating the other chapters of the book, as well as work elsewhere. It is important to note, however, that their model is explicitly a problem-solving model and that not all approaches assume this stance, such as Edward Gross's model in "Work, Organization and Stress."

Richard S. Lazarus's excellent chapter on "Cognitive and Personality Factors Underlying Threat and Coping" reexamines the traditional view in stress causal analysis, namely, that emotions cause cognitive and behavioral reactions. He turns it around so that emotional reactions are effects rather than causes, and these in turn should be viewed as being heavily dependent on cognitive processes; thus, cognitive processes → emotion → organization of behavior. Then the important theoretical and research problems become concerned with isolating the different cognitive processes that underlie threat, its appraisal, and those conditions that determine the cognitive processes.

Andrew Crider's paper on "Experimental Studies of Conflict-produced Stress" argues convincingly that behavioral and physiological reactions to conflict or aversive events are independently organized systems, not necessarily positively correlated, and that the use of physiological measures as "indices" of stress may be a dubious procedure. Such a conclusion has wide ramifications for many of the assumptions under which other research studies of stress are conducted and link various variables.

John Cassel's "Physical Illness in Response to Stress" aptly reviews the evidence supporting the contention that social factors are important determinants of health status, and he critically designates steps and means to augment current knowledge and to increase the efficiency of future research endeavors. Both he and E. Gartly Jaco in "Mental Illness in Response to Stress" broach the problems of specificity in stress studies, that is, link with stress precisely defined dependent variables vis-à-vis some clear, testable causal pathway.

The McGrath volume is the product of a three-day conference con-

cerned with identifying crucial issues in the area of social-psychological stress and then, in turn, seeking promising research approaches to these issues. All of the original papers given at the conference are included.

McGrath's volume is more difficult to read because some papers assume that the reader is quite familiar with the psychological stance and laboratory studies. However, most of the book should be readily understood and appreciated. While sociologists may find certain chapters less interesting than psychologists or social psychologists would, the majority of them merit careful scrutiny by sociologists looking for a well-balanced, in-depth grasp of current stress research.

Although the editor does not attempt to divide his volume into any major parts or themes, the first five chapters accomplish the tasks of isolating the crucial issues raised at the conference and tying together the remainder of the book. These chapters were written by McGrath, and, although he makes no claim for personal origin, they are superbly written. Chapter 2, "A Conceptual Formulation for Research on Stress," presents a pragmatic decision to shift the focus from the question What is stress? to the question What is stress research? This provides a frame of reference for considering the total stress research problem in terms of a conceptual paradigm involving a sequence of events. McGrath's chapter 2 and Scott and Howard's chapter in the Levine and Scotch volume must be read for comparisons and for an introduction to the area of stress research. Chapters 3 and 4 set out the major substantive issues of time, setting, and the coping process, as well as the major methodological issues. Chapter 5 undoubtedly contains the best and most extensive bibliography on stress studies available. It includes a sample of 200 research studies integrated and cross-classified around central theoretical propositions, varieties of settings for stress research, methods used, indices of stress used, levels of analysis, etc. David Mechanic's "Some Problems in Developing a Social Psychology of Adaptation to Stress" underscores the complex effects of the temporal dimension in the coping process and questions the value of retrospective self-report measures of stress, coping mechanisms used, etc. Karl E. Weick also addresses himself to, among other things, this issue in "The 'Ess' in Stress· Some Conceptual and Methodological Problems." Robert L. Kahn, in "Some Propositions toward a Researchable Conceptualization of Stress," is one of the few authors of both volumes to introduce clearly and explicitly the dimensions "overload" and "underload" of stress stimulation and potential consequences for the individual or organism. Kahn, again in conjunction with John R. P. French, Jr., spells out graphically a theoretical model of factors involved in adjustment to role conflict and ambiguity in "Status and Conflict: Two Themes in the Study of Stress."

In summary, there is much in both of these volumes to debate, disagree with, and carefully ponder. This is as it should be, for the area of stress studies is still at a very youthful, challenging, and provocative stage.

American Drinking Practices: A National Study of Drinking Behavior and Attitudes. By Don Cahalan, Ira H. Cisin, and Helen M. Crossley. New Brunswick, N.J.: Publication Division of Rutgers Center of Alcohol Studies, 1969. Pp. xxvi+260. $9.50.

The Prevention of Drinking Problems: Alcohol Control and Cultural Influences. By Rupert Wilkinson. New York: Oxford University Press, 1970. Pp. xv+301. $10.00.

The Treatment of Alcoholics: An Evaluative Study. By Sidney Cahn. New York: Oxford University Press, 1970. Pp. viii+246. $7.50.

George L. Maddox

Duke University

The subject is drinking behavior in the United States. These books variously provide descriptions of our drinking styles, suggest explanations of why Americans experience so much trouble due to drinking, and discuss what is being done or might be done to reduce that trouble. In balance, each book in its own way makes a useful contribution to our understanding of the most commonly used and abused recreational drug in our society.

Sustained interest in drinking behavior has been uncommon among scientists in general and sociologists in particular. This neglect is interesting insofar as few issues have dominated the American political scene so consistently over the past century as Prohibition. And by any objective criteria, documented trouble due to drinking and the prevalence of excessive drinking suggest a major social problem. The combined social problems associated with all other drugs are simply not in the same league with alcohol-related problems.

Perhaps the magnitude of these problems and the emotional freight they carry provide a partial explanation for the neglect of scientists; neither the scientists nor the society which provides them resources has shown much enthusiasm for objective appraisal of the high risk associated with the use of a commonly available legal drug. Thus, in spite of the fact that an estimated 5 million adults suffer very serious physiological, sociological, and psychological damage in which alcohol plays a prominent role, no one expects a warning from the surgeon general to the effect that drinking may be dangerous to health. And it is not unusual for discussions of potentially dangerous drugs to omit reference to alcohol altogether. In the face of such ambivalence, we are perhaps fortunate to have any research at all on drinking behavior.

American Drinking Practices provides the most recent and most comprehensive available survey of who uses our preferred drug, when, where, with whom, and with what effect. The characterizations which emerge are based on a national probability sample ($N = 2{,}746$, Hawaii and Alaska excluded) of persons twenty-one and over. The major findings are rarely

surprising, but some of the details are interesting. While only a minority (32 percent) of Americans are abstainers and an additional 43 percent drink no more than lightly and occasionally, a substantial majority of all drinkers report that drinking does more harm than good. Ambivalence about drinking appears to be endemic. There is also convincing evidence that dichotomizing the population as drinkers and nondrinkers is not particularly useful. A majority of individuals (51 percent) reported some change over time in their abstinence or drinking. Although drinking careers cannot be constructed from cross-sectional data, many abstainers are clearly ex-drinkers and all drinkers were once abstainers. High-status individuals produce more than their share of drinkers, and low-status individuals are more likely to be abstainers. But status differences appear to be irrelevant among heavy drinkers (three or more times a day, typically in quantity). Heavy drinkers constituted 12 percent of the sample and about 18 percent of all drinkers. Among drinkers, self-reports on reasons for drinking suggest that about 29 percent stress the escapist and reality-modifying aspects of alcohol as a drug in contrast to a recreational beverage. Nine percent of the sample were "heavy escape" drinkers (13 percent of the male and 5 percent of the female drinkers).

The authors avoid labeling the "heavy escape" drinker as *alcoholic*. However, their estimate is generally consonant with other evidence on the incidence of persons with serious trouble associated with drinking in our society.

American Drinking Practices is recommended as an illustration of well-conceived survey research. Its limitations are explicit: cross-section data can only hint at social process and change; description and reported associations only hint at explanation of deviant drinking practices; some subsamples are too small to be useful (e.g., Negroes). Strengths, however, far outweigh limitations.

The social problems associated with drinking, which are largely implicit in *American Drinking Practices*, are explicit in the other two volumes. *The Treatment of Alcoholics* and *The Prevention of Drinking Problems* are the products of the Cooperative Commission on the Study of Alcohol, which was funded by the National Institute of Mental Health at the beginning of the last decade. Working initially at Stanford, staff members of the commission produced *Alcohol Problems: A Report to the Nation* (New York, 1967) to document the incidence and social costs of inebriety in its various forms in the United States. Cahn in *The Treatment of Alcoholics* focused his attention on the most dramatic and obvious losers, those designated as alcoholics. Cahn does no worse than others in failing to develop a convincing definition and explanation of the phenomenon of interest; he presents an eclectic view (psychodynamics, learning theory, anomie, and social labeling) of a serious behavior disorder which is "a component in a problem syndrome." Thus, partially armed, he reports visiting twelve SMSAs and forty-five major cities in twenty states and reviewing thirty community studies in search of a definitive picture of the treatment

of alcoholics. We are hardly surprised by the lack of consensus about how to identify and treat such an ambiguous condition, but the extent of the lack of consensus and the ineffective programs recorded is a sad commentary on our ability to deal with alcohol-related social problems. The author seems reluctant to be as explicitly evaluative as his subtitle suggests he will be. One simply looks in vain for any indication that law agencies, state hospitals, community clinics, medical practitioners, and other consulting professionals generally have been very clever in treating, much less preventing, the persistent and troublesome inebriety of a substantial number of drinkers.

The audience which will find this book interesting is not easy to identify. Sophisticated readers who know the relevant literature will find no new synthesis of fact and theory and will find the 1963 statistics on hospital admissions quite out of date. The novice will probably find the lengthy catalog of agencies and programs which constitute the alcoholism industry confusing. Everyone will look in vain in this book for a framework for evaluating the treatment of alcoholism.

The Prevention of Drinking Problems is in fact two books in one. Wilkinson believes that particular social and cultural factors contribute to the high incidence of drinking problems in the United States. Specifically, he faults the endemic emotionalism and ambivalence. Ambiguous social rules, he contends, encourage the separation of eating and drinking, winking at inebriety, and the high consumption of high-alcohol-content beverages. Relegating to appendixes the evidence which convinces him that such factors are important for development, the author presents a readable description of the alcohol industry, the punitive legal environment in which alcohol is distributed and consumed, and the inadequacies of education about alcohol in our society. His advice is sensible and calls attention to the possible advantages of a uniform lower legal drinking age, a tax policy which favors low-alcohol-content beverages, educational campaigns to encourage the association of eating and drinking, and unambiguous rejection of public inebriety, especially when driving automobiles is concerned.

The thesis which underlies these recommendations is that access to high-proof beverages, drinking styles which separate drinking from eating, and ambiguous social rules which permit joking about public inebriety are conditions associated with high risk for drinkers regardless of what one assumes about their psychological vulnerability. In appendixes, Wilkinson reviews some interesting Swedish data regarding the effects of control systems on drinking behavior and an extensive literature on drinking customs in various groups associated with sobriety. While the review is interesting, the result is far short of a justification for the specific recommendations made. The book is provocative but not more than this.

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1. Date of filing: 29 October 1970
2. Title of publication: AMERICAN JOURNAL OF SOCIOLOGY
3. Frequency of issue: bi-monthly
4. Location of known offices of publication: The University of Chicago Press, Journals Department, 5801 S. Ellis Ave., Chicago, Illinois 60637.
5. Location of the headquarters or general business offices of the publisher The University of Chicago Press, Business Office, 11030 S Langley, Chicago, Illinois 60628.
6. Names and addresses of publisher, editor, and managing editor: Publisher The University of Chicago Press, 5801 S. Ellis Ave., Chicago, Illinois 60637, Editor: C. Arnold Anderson, The University of Chicago Press, 5801 E Ellis Ave., Chicago, Illinois 60637; Managing editor Florence Levinsohn, The University of Chicago Press, 5801 S Ellis Ave., Chicago, Illinois 60637
7. Owner: The University of Chicago, Chicago, Illinois 60637
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